ANNALS OF PHILOSOPHY;

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NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

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ERRATA IN VOL. XV.

Page 99, line 14, *read* $n \Delta \varphi$, instead of $\Delta \varphi$.

line 3 from bottom, *read* $\varphi + n \, d \, \varphi$, instead of $n \, d \, \varphi$.

101, line 16, *read* $6 \, w^2 \, a^2$, instead of $6 \, w^2 \, x$.

line last, *read* $\Delta \, x^2$, instead of Δ^2.

102, line 20, *read* $w (\log. \, x + 1) + w + \&c.$

103, line 9, *read* $(l \, a)^2$, instead of $(l \, a^2$.

ANNALS

OF

PHILOSOPHY.

JANUARY, 1820.

ARTICLE I.

On the Solubility of the Salts in Water. By M. Gay-Lussac. *

ONE is astonished, on perusing the different chemical works, at the inaccuracy of our knowledge, respecting the solubility of the salts. They satisfy themselves with the common observation that the salts are more soluble in hot than in cold water, and with the solubility of a few of them at a temperature usually very uncertain; yet it is upon this property of salts that their mutual decomposition, their separation, and the different processes for analyzing them depend. As a chemical process, the solution of the salts deserves peculiar attention; for though the causes to which it is due are the same as those which produce other combinations, yet their effects are not similar. It is to be wished that this interesting part of chemistry, after remaining so long in vague generalities, may at last enter the domain of experiment, and that the solubility of each body may be determined not merely for a fixed temperature, but for variable temperatures. In the natural sciences, and especially in chemistry, general conclusions ought to be the result of a minute knowledge of particular facts, and should not precede that knowledge. It is only after having acquired this knowledge that we can be sure of the existence of a common type, and that we can venture to state facts in a general manner.

The solubility of a body in water depends upon two causes, affinity and heat, or more exactly the affinity of a salt for water varies with the temperature. Lavoisier, whose philosophic

spirit embraced all the parts of chemistry, is the first person who explained in a satisfactory manner the influence of heat in saline solutions. " If," for example, says this illustrious chemist, " a salt is very little soluble in water, and very fusible by heat, it is clear that such a salt will be very little soluble in cold water, but very soluble in hot water. Such is the nitrate of potash, and above all the hyperoxymuriate of potash. If another salt, on the contrary, is at the same time little soluble in water and caloric, it will be little soluble both in cold and hot water; and the difference will not be considerable. This is the case with sulphate of lime."

" We see then that there is a necessary relation between these three things ; the solubility of a salt in cold water, its solubility in boiling water, the degree at which it melts by heat alone, and by the assistance of water. That the solubility of a salt in hot and in cold water is so much the greater the more soluble it is in caloric, or, which comes to the same thing, the lower the temperature at which it is disposed to melt."—(Traité Elementaire de Chimie, ii. 39.)

These principles, when we consider only some particular examples, appear just and very clear. We conceive in fact that if a salt melts at 212°, it will mix at that temperature with water in every proportion, however little affinity it may have for that liquid; but they present a great number of exceptions, and to determine their degree of accuracy and their generality, it is indispensable to examine the solubility of a great number of bodies.

The determination of the quantity of salt which water can dissolve is not a very difficult process. It consists in saturating the water exactly with the salt whose solubility we wish to know at a determinate temperature, to weigh out a certain quantity of that solution, to evaporate it, and weigh the saline residue. However, the saturation of water may present considerable uncertainty, and before going further, it is proper to examine the subject.

We obtain a perfectly saturated saline solution in the two following ways; by heating the water with the salt, and allowing it to cool to the temperature whose solubility is wanted ; or by putting into cold water a great excess of salt, and gradually elevating the temperature. In each case, it is requisite to keep the final temperature constant for two hours at least, and to stir the saline solution frequently, to be quite sure of its perfect saturation. By direct experiments made with much care, I have ascertained that these two processes give the very same result ; and that of consequence they may be employed indifferently. I shall mention a few.

I carried into the caverns below the observatory, where the annual temperature does not vary more than $\frac{1}{100}$th of a degree, two solutions of nitre and of sulphate of soda saturated at the

temperature of 25° (77° Fahr.) I placed at the same time in the same place two flagons, one containing crystals of nitre, the other crystals of sulphate of soda, into which I poured water of the temperature of 8° (46·5° Fahr.); so that the liquid did not cover the salt. After an interval of a fortnight, I evaporated known quantities of each solution, and obtained the following results :

Temperature of the cavern 11·67° (53° Fahr.)

Saturated solution of nitre,

By cooling, 100 water contain........ 22·24 salt
By simple contact .... 22·22

Saturated solution of sulphate of soda,

By cooling, 100 water contain........ 10·11 salt
By simple contact .... 10·14

Other experiments made by the processes which I have usually employed to form saturated solutions gave me results whose differences, in general very small, were sometimes on one side and sometimes on the other. I admit then as a certain fact that water for a determinate temperature comes to the same degree of saturation, either by allowing the excess of salt which it contains in solution to precipitate by cooling, or by immediately dissolving the same salt, provided it remain for a sufficient time in contact with it. This result might have been foreseen, for the circumstances in both cases are rigorously the same. I may likewise remark, that the volume of crystals which form in a solution, or which are put into it, has no sensible influence on the term of saturation. This is the consequence of the nature of chemical affinity which acts only at distances infinitely small.

Yet Dr. Thomson found that water retains more oxide of arsenic, when saturated by cooling, than when put in contact with the oxide without any elevation of temperature ; but the reason I am persuaded was, that he employed too little oxide of arsenic relatively to the water, and that he did not prolong the contact sufficiently. We perceive in fact, on a little reflection, that saturation follows in its progress a decreasing geometrical progression, and that the time necessary for completing it depends upon the surface of contact of the solvent and the body to be dissolved.

It happens often that the solution of a salt which does not crystallize, and which, for that reason, we consider as saturated, yields saline molecules to the crystals of the same nature plunged into it ; and it has been concluded from this, that the crystals of a salt impoverish a solution, and make it sink below its true point of saturation. The fact is certain ; it is even very general ; but I am of opinion that it has been ill explained.

Saturation in a saline solution of an invariable temperature is

the point at which the solvent, always in contact with the salt, can neither take up any more, nor let go any more. This point is the only one which should be adopted, because it is determined by chemical forces, and because it remains constant as long as these forces remain constant. According to this definition, every saline solution which can let go salt without any change of temperature is of necessity supersaturated. I shall now show that in general supersaturation is not a fixed point, and that the cause which produces it is the same as that which keeps water liquid below the temperature at which it congeals.

When a liquid, or even an elastic fluid, is to become solid, the change does not always take place at the temperature at which it ought to happen. Water, for example, whose freezing point is 32°, may, in suitable circumstances, remain fluid 10° or 20° below that point; and its boiling, which in a metallic vessel takes place at 212°, is very sensibly retarded in vessels of glass. The same liquid may likewise retain in solution a greater quantity of carbonic acid than corresponds with the pressure. The effects of this kind are very numerous; their intensity in determinate circumstances ought to be constant; but as they appear to depend upon the inertia of the molecules, which is general in a very weak force, and which yields to the slightest effort, we are never sure of coming to the point at which this intensity is a maximum; for example, we have observed that in some experiments water remained liquid 18° below its freezing point, but nothing indicates that it may not preserve it in a much more considerable cold. By inertia of molecules, which is necessarily a vague expression, we must understand a resistance to a change of state or equilibrium which may be produced by different causes; such as the difficulty of a change of place in the molecules in a medium perfectly homogeneous; the viscosity of the solvent; the conduction of heat, which, by opposing a resistance to the disengagement or the absorption of heat, may maintain the equilibrium of the molecules; and, perhaps, also an electric influence.

It is certain at least that all the effects of which we have just spoken may be prevented or destroyed by causes which appear strangers to affinity. Thus water congeals always at 32°, and boils at 212° nearly in glass vessels, it does not take an excess of carbonic acid, or it lets that excess go, when it is agitated. It is true that a piece of ice introduced into water cooled down below 32° will infallibly occasion its crystallization, in consequence of the reciprocal affinity of the molecules of water, which is greater in the solid than the liquid state. But this is only an additional method of destroying the inertia; and frequently inert bodies, particularly when they have asperities, produce the same effect.

Supersaturated saline solutions have a very great analogy with water cooled down below the freezing point, and every thing

that has been just said may be applied to them. We may obtain
these solutions by evaporating very slowly a portion of the sol-
vent; but it answers better to cool slowly solutions already satu-
rated; for some salts, as the sulphate and carbonate of soda,
the cooling may be considerable before the crystallization begins;
but in general it ought to be very little. The general cause
producing supersaturation being evidently the same for each
salt, it will be sufficient to observe the effects in those which
show them with the greatest intensity. I shall take, as an
example, the carbonate of soda.

A supersaturated solution of this salt crystallizes, just like
water remaining liquid below the point of congelation, either by
agitation, or the immersion of a crystal of carbonate of soda, or
of a foreign body; and, as is the case with water, we cannot
assign the limit at which the supersaturation stops short. This
limit, in each experiment, is entirely accidental. It depends
upon the nature of the vessel, on its smoothness, its conduction,
the agitation of the air. But as we determine crystallization in
a supersaturated solution of carbonate of soda by a slight agita-
tion, it is obvious that this supersaturation does not depend on
affinity, but on a force purely mechanical; for motion cannot of
itself produce chemical effects.

There are a great many saline solutions which crystallize as
soon as they lose a portion of the solvent, or their temperature is
somewhat lowered; and it would be difficult by the means
pointed out above to know whether they can be supersaturated;
but the phenomena of crystallization leave no doubt on that
head.

If we consider a saline solution in which some scattered crys-
tals have formed, if it be exposed to evaporation, these crystals
will increase without the formation of any new ones, or at least
this will often happen. But evaporation taking place only at the
surface of the liquid, it follows of necessity that the saline mole-
cules, which have lost a portion of their solvent, still continue in
solution till they approach the crystals which seize upon them;
for if they precipitated as soon as they lose their solvent, we
could not conceive the regular increase of the crystals. This is
what actually happens, when the evaporation is too rapid, rela-
tively to the supersaturation which the solution is capable of
assuming. In that case, a multitude of small crystals are depo-
sited on the sides of the vessel containing the saline solution.

Supersaturation does not appear to depend on the affinity of
the salt for the solvent, for it is very far from being proportional
to it. This is a further proof that it is owing to a particular
disposition of the saline molecules, in consequence of which they
resist more or less a change of state.

I have chosen water as an example to explain by analogy the
supersaturation of saline solutions, because the permanence of

its liquidity below the point of congelation cannot be ascribed to affinity, as in the saline solutions, where it may be supposed that the supersaturation is owing to the affinity of the salt for its solvent; but as the effects are the same in circumstances absolutely similar, it is very probable that their causes are the same.

There has been long entertained an opinion respecting the permanence of the saturation of saline solutions which I have never adopted; because it does not appear to me sufficiently demonstrated. The opinion is, that water saturated with a salt is capable of depositing a portion of it when left at rest, though its temperature does not alter. But M. Beudant in his memoir on the causes which may produce changes in the form of the crystals of the same mineral, has cited several facts in support of this opinion, which he appears to adopt. I consider it, therefore, as necessary to discuss it here.

" I have remarked," says M. Beudant (Ann. de Chim. et de Phys. viii. 15), " that very regular crystals may form without any evaporation whatever in solutions otherwise very dilute ; but it does not appear that all the salts are in this state. To satisfy myself on the subject, I placed dilute solutions of different salts, all at the same degree of density, in flaggons completely filled, and well stopped, which I left in a press. On visiting them long after, I observed that they were all equally full, and that consequently there had been no evaporation ; but in several of them the salts had crystallized. I remarked that it was precisely those which had the greatest degree of cohesion, as the sulphate of potash, alum, borax, muriate of barytes &c. ; while those whose cohesion was weaker, as nitrates of potash and ammonia, sulphates of ammonia and iron, muriate of soda, &c. had not crystallized."

In the Ann. de Chim. et de Phys. vii. 79, 1 have already stated experiments in my opinion very decisive, showing that saturated saline solutions, whose temperature is constant, do not deposit salt; and that they remain homogeneous through their whole extent; but since that time I have made new experiments to answer all objections that could be advanced, and the result of them has been precisely the same as that of the former ones.

I took two glass tubes, two metres in length, and three centimetres in diameter. I put into one a solution of nitre saturated at the temperature of the caverns under the Observatory, and into the other a solution of sea salt equally saturated. Two other tubes were filled with similar solutions, in which there was not more than four per cent. of each salt. These tubes well closed remained six months in the caverns of the Observatory in a vertical situation. At the end of that time I determined by evaporation the quantity of salt contained in the water of the upper and lower part of each tube, and I found that the solutions

were perfectly homogeneous. I shall add likewise that crystals of nitre suspended below the surface of a saturated solution of that salt, underwent no sensible diminution, during an interval of more than two years, that they have remained in the constant temperature of the caverns of the Observatory.

The facts which I have just stated are opposite to those of M. Beudant; and if they were produced by the same cause, they could not exist at the same time. It remains for me to show that in fact they are owing to different causes.

M. Beudant does not give sufficient details respecting his experiments; but they can be perfectly well imitated by taking saline solutions saturated or not, letting them cool for some time, and then bringing them back to their original temperature. If the cooling has been sufficiently low to supersaturate the solutions, they will have crystallized in their lower parts. If now the temperature be raised, the crystals will not be completely dissolved provided we do not agitate; because the liquid in contact with them will acquire by saturating itself more density than the upper portions. It will, therefore, remain at the bottom of the vessel, and will prevent the crystals from dissolving. The solutions which did not crystallize in M. Beudant's experiments were not sufficiently saturated to be carried beyond the limit of saturation by the cooling which they experienced. It appears to me certain from these facts, that the crystallizations which M. Beudant observed were entirely owing to the cooling of the saline solutions.

I shall now give an account of the experiments which I have made on the solubility of the salts.

Having saturated water with a salt at a determinate temperature, as I have explained above, I take a matrass capable of holding 150 to 200 grammes of water, and whose neck is 15 to 18 centimetres in length. After having weighed it empty, it is filled to about a fourth part with the saline solution, and weighed again. To evaporate the water, the matrass is laid hold of by the neck by a pair of pincers, and it is kept on a red-hot iron at an angle of about 45°, taking care to move it continually, and to give the liquid a rotatory motion, in order to favour the boiling, and to prevent the violent bubbling up, which is very common with some saline solutions, as soon as, in consequence of evaporation, they begin to deposit crystals. When the saline mass is dry, and when no more aqueous vapours are driven off at a heat nearly raised to redness, I blow into the matrass by means of a glass tube fitted to the nozzle of a pair of bellows, in order to drive out the aqueous vapour which fills it. The matrass is then allowed to cool, and weighed. I now know the proportion of water to the salt held in solution, and this is expressed by representing the quantity of water to be 100. Each of the following results is the mean of at least two experiments:

Solubility of Chloride of Potassium.

Temperature centigrade.	Chloride dissolved by 100 water.
0·00°	29·21
19·35	34·53
52·39	43·59
79·58	50·93
109·60	59·26

If we construct these results, taking the degrees of temperature for abscissas, and the quantities of salt dissolved by 100 water for ordinates, we shall see that they can be represented by a straight line. To find its equation, I have supposed the ordinate 34·53 corresponding to 19·35° to be constant, because the experiments on solubility at mean temperatures should in general be more accurate than those made at temperatures at a great distance from them, and a straight line was made to pass successively by the extremity of each ordinate representing the solubility. There resulted for the tangent of the angle which the straight line makes with the line of abscissas the following values :

$$0·2749; \ 0·2742; \ 0·2723; \ 0·2740;$$

the mean of which is 0·2738. Of consequence, the equation of the line of solubility of the chloride of potassium will be :

$$y = 0·2738 \ x° + 29·23$$

It may be employed with certainty to find the solubility of chloride of potassium at all temperatures comprehended between the two extremes 0° and 109·6°; but in all probability it would not serve for temperatures much more elevated, or much lower. This equation is constructed in Plate C, under the name of chloride of potassium. By means of the division of the lines on which are counted the temperatures and solubilities, it will be easy to find without calculation the corresponding solubility at a determinate temperature.

Solubility of Chloride of Barium.

Temperature centigrade.	Salt dissolved in 100 water.
15·64°	34·86
49·31	43·84
74·89	50·94
105·48	59·58

the equation of the line of solubility is,

$$y = 0·2711 \ x° + 30·62$$

In these experiments, the chloride of barium is supposed to be anhydrous ; but as when it is crystallized it retains two proportions of water, 22·65, for one of chloride, 131·1, we must of

5 80 85 90 95 100 105 80

80
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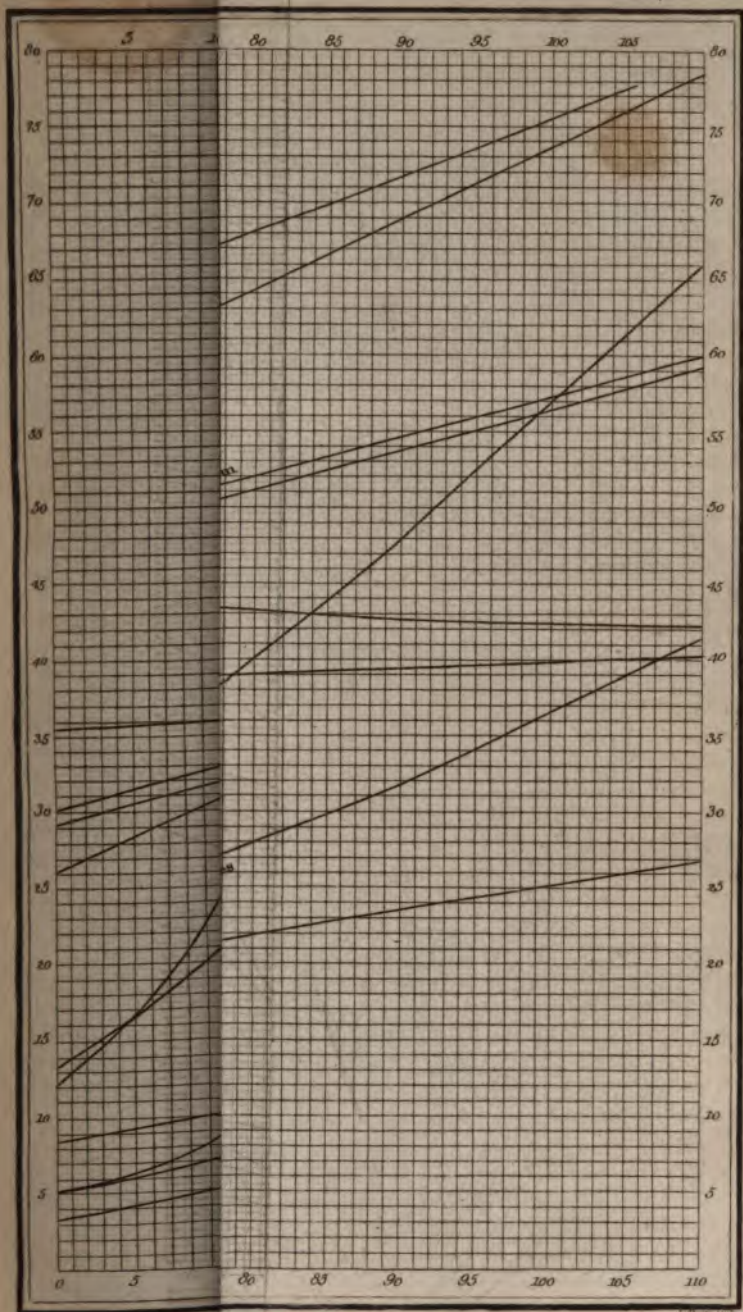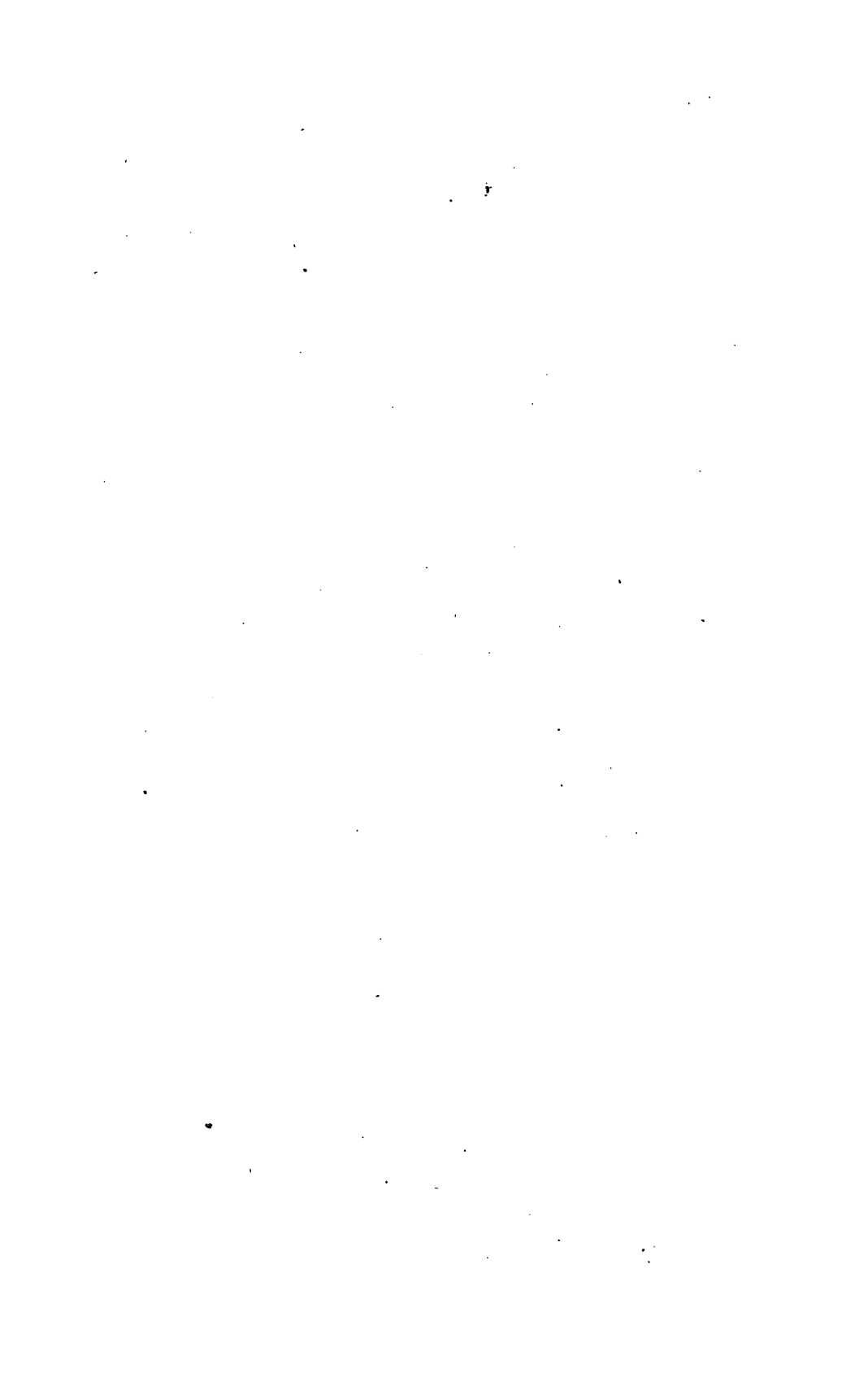
necessity, in order to compare its solubility with that of other salts, increase each number of solubility by the same number multiplied into the ratio of 22·65 to 131·1, and diminish by as much the quantity of water. On making this correction, the preceding results will be changed into the following:

Temperature.	Salt dissolved in 100 water.
15·64°..................	43·50
49·31	55·63
74·89	65·51
105·48	77·89

These results are represented by a curve deviating very little from a straight line. They are represented on the plate.

Solubility of Chloride of Sodium.

Temperature.	Salt dissolved in 100 water.
13·89°..................	35·81
16·90	35·88
59·93	37·14
109·73	40·38

The line of solubility of this chloride is not a straight line; for the equation of a line passing by the two extreme ordinates, 35·81 and 40·38, is $y = 0·04768\ x° + 35·15$, and the solubility calculated from this equation for the temperature 59·93° is 38·01 instead of 37·14, which experiment gives. At the temperature of 0°, I found a solubility a little greater than at 13·89°, and I intend to make new experiments to investigate this anomaly.

Solubility of Sulphate of Potash.

Temperature.	Salt dissolved in 100 water.
12·72°..................	10·57
49·08	16·91
63·90	19·29
101·50	26·33

The line of solubility is a straight line whose equation is

$$y = 0·1741\ x° + 8·36$$

Solubility of Sulphate of Magnesia.

Temperature.	Salt dissolved in 100 water.
14·58°..................	32·76
39·86	45·05
49·08	49·18
64·35 ·	56·75
97·03 ·	72·30

The line of solubility is a straight line whose equation is

$$y = 0·47816\ x° + 25·76$$

The sulphate of magnesia is here supposed anhydrous; but as

it crystallizes retaining seven portions of water, 79·3, for one proportion of salt, 74·6, each number which expresses the solu-. bility, must be increased by this number multiplied by the ratio of 79·3 to 74·6, and the corresponding quantity of water diminished as much. We shall thus have for the solubility of crystallized sulphate of magnesia the following results :

Temperature.	
14·58°	103·69
39·86	178·34
49·08	212·61
64·35	295·13
97·03	644·44

These results are no longer proportional to the temperatures; they augment in a much greater ratio.

Solubility of Sulphate of Soda.

	Salt soluble in 100 water.	
Temperature.	Anhydrous.	Crystallized.
0·00°	5·02	12·17
11·67	10·12	26·38
13·30	11·74	31·33
17·91	16·73	48·28
25·05	28·11	99·48
28·76	37·35	161·53
30·75	43·05	215·77
31·84	47·37	270·22
32·73	50·65	322·12
33·88	50·04	312·11
40·15	48·78	291·44
45·04	47·81	276·91
50·40	46·82	262·35
59·79	45·42	
70·61	44·35	
84·42	42·96	
103·17	42·65	

We see by these results that the solubility of sulphate of soda follows a very singular law. After having increased rapidly to about the temperature of 33°, where it is at its maximum, it diminishes to 103·17°, and at that point it is nearly the same as at 30·5°. The sulphate of soda presents the second example of a body whose solubility diminishes as the temperature augments; for Mr. Dalton has already observed the same property in lime. In the plate, the line of solubility of anhydrous sulphate of soda is traced. It consists of two branches convex towards the axis of the abscissas, and having a point of contrary flexure corresponding nearly to the temperature of 33°. It was not possible to represent more than a small portion of the line of solubility of crystallized sulphate, on account of the great extent of the ordinates; besides, this line could not be of use beyond 50°, because

the sulphate of soda does not then retain a quantity of water as considerable as in low temperatures.

Solubility of Nitrate of Barytes.

Temperature.	Salt dissolved in 100 water.
0·00°	5·00
14·95	8·18
17·62	8·54
37·87	13·67
49·22	17·07
52·11	17·97
73·75	25·01
86·21	29·57
101·65	35·18

Solubility of Nitre.

Temperature.	Salt dissolved in 100 water.
0·00°	13·32
5·01	16·72
11·67	22·23
17·91	29·31
24·94	38·40
35·13	54·82
45·10	74·66
54·72	97·05
65·45	125·42
79·72	169·27
97·66	236·45

Solubility of Chlorate of Potash.

Temperature.	
0·00°	3·33
13·32	5·60
15·37	6·03
24·43	8·44
35·02	12·05
49·08	18·96
74·89	35·40
104·78	60·24

The lines of solubility of these three salts are represented in the plate. They show at once to the eye that the solubility of each salt, especially nitre, increases at a great rate. It would have been possible to have represented them by algebraic formulas, but their graphical lines have the advantage of giving immediately, and without calculation, and with almost as great precision, the solubility for all temperatures between which the experiments were made. I intend to give in a second paper the solubility of a greater number of salts, and to present them under the form of tables for every five degrees of temperature at least.

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The results which I have obtained, though not numerous, are sufficient to show that if we cannot hope to reduce the solubility of the salts to general principles, it deserves at least a particular attention, expressly in consequence of the anomalies which it exhibits. I abstain at present from drawing any consequence; I shall wait till I obtain the assistance of new experiments.

ARTICLE II.

Further Observations on the Use of Gauze Veils as Preservatives from Contagion. By Mr. Bartlett.

(To Dr. Thomson.)

SIR,   *Buckingham, Oct.* 11, 1819.

WHEN I suggested, through the medium of your *Annals*, the use of *gauze veils* as preservatives from contagion, I was not aware that M. Rigaud de l'Isle had, in a memoir "On the Physical Properties of ·Bad or Unwholesome Air," the *aria cattiva* of the Italians, read at a meeting of the Royal Academy of Sciences at Paris, recommended to those exposed to a deleterious atmosphere some simple thing as a screen to be placed before the organs of respiration so as to intercept the insalubrious particles mingled with the air they breathe. His paper, although confined to those local causes of epidemics which prevail in districts to which his personal observation had extended, is so replete with information, and so corroborative of the efficacy of the measure which I am desirous to extend to animal effluvia, as well as to miasmata arising from vegetable decomposition, that I shall, perhaps, be pardoned if I avail myself of some of the facts which he adduces. I am led to intrude these remarks upon your notice at this time more particularly from the circumstance of my " suggestion " having been condemned by a medical work * with an asperity, " not to use a harsher term," I was not altogether prepared to expect, nor do I feel conscious of having at all deserved it. But the use of

"———— *scalping knives instead of pens,*"

may, without much impropriety, be conceded to the faculty; I am, therefore, content to allow this great "*northern light,*" the reviewer, (who, by the by, gravely tells the world that contagion is an "*animal product,*" and afterwards expresses, in the very same page, his ignorance of its properties! †) the triumph of his irony; while I will oppose to his " want of faith " that " test of

* The Edinburgh Medical and Surgical Journal, No. 61, p. 621, 622.
† Ibid. p. 620.

truth," experience, which he so confidently challenges. I will now pursue the arguments of M. Rigaud de l'Isle. "*Miasmata,*" he says, "*possess such a gravity that they can never rise in the atmosphere unless assisted by a lighter body, which carries them into it.*" In support of this, he says, the air which is very unhealthy at Montalto, Corteno, and along all that coast, stretching to the south as far as Terracina, becomes salubrious on Mount Argentaro, which rises above Orbitello. The villages of la Tolfa, and the habitations situated above Civita Vecchia on the Cimic hills, afford a very agreeable and healthy abode, though situated in the centre of that region of desolation. The same is the case when we rise above the village of St. Felice on the mountain of Circe to the palace of Theodoric above Terracina ; to the villages of Segga and Sermoneta, perched perpendicularly above the Pontine marshes on the rocks of the Lepine mountains ; also at Monte Fiascone above the lake of Bolsena, above the villages of Valentano, Capo di Monte, Martha, &c. A little further eastward, on the insulated rock of St. Orestes, the inhabitants of the village, which is built on its side, enjoy the best health ; if they descend, disease attacks them, and common fevers make their appearance ; and a little lower down, for instance at Sandreva, they will have putrid fevers ; and still lower down, they will die. The observations of some eminent travellers support these remarks.

"Miasmata," he continues, "have no smell by which they can be distinguished. They may be separated from the odorous substances with which they seem to be most intimately blended." Of the peculiar odour emitted by stagnant waters, he adds, it has something disagreeable and sickly, which seems to warn us not to approach places where it is perceived ; it may, however, be inhaled without any ill effect in certain seasons of the year. I have myself been several times exposed to it. In 1810 and 1811, in passing the numerous ponds which cover the sea coast of the ecclesiastical state, at Maccharese, Ostia, Foliguano, in the Pontine marshes, which I have repeatedly traversed in various directions, I have always perceived this peculiar smell, without sustaining any inconvenience from it. The following year, on the contrary, on a very hot day in the beginning of September, among the ponds of Vauvert, between St. Giles and Aigues Mortes, in Languedoc, I was suddenly seized with nausea and a feeling of sickness, which lasted several days, though I remarked at the time that no kind of odour was emitted by the marsh. He asserts, from experience, that "it is much more dangerous to inhale bad air in the night than in the day time. All the hours of the day or of the night are not attended with equal risk. The least critical moment is when the heat is greatest and the sun highest above the horizon. The most dangerous is that which accompanies the setting and that which precedes the rising of the sun." This observation, which applies

to all times and places, proves to demonstration the *union of miasmata* with *aqueous vapours;* the former are heavy ; the latter, possessing extreme levity and dilatibility, lend them wings. Rarified in the middle of the day by the heat, the more elastic and lighter vapours must then occupy more space in the atmosphere; the miasmata which they carry with them must also, at such times, be more widely diffused ; we do not, therefore, inhale them in such large doses in the same volume of air, and consequently cannot in those hours be so much affected by them. M. Rigaud de l'Isle proves that miasmata are much less subtle than the air, or the principle of smells ; since air and odorous effluvia penetrate into every place, whereas miasmata are stopped and expelled by various obstacles. *" The interposition of a forest, a mountain, a high wall, or even of a mere cloth, may also co-operate in this separation, and preserve us in a variety of circumstances from the pernicious effects of the air charged with deleterious miasmata."*

The arguments of the non-contagionists have, I conceive, been too ably refuted to require an additional exposition of their fallacy. Were, however, further proofs necessary, the circumstances which attended the too early removal of the restrictions upon the inhabitants of Marseilles, when the plague raged there in 1720 ; and the fatal effects which followed the temerity of the students of medicine at Edinburgh, some years since, during the prevalence of the Infirmary fever, would sufficiently demonstrate it. It is also, I think, equally clear that contagion can be communicated by means of the respiratory organs only ; this opinion experience hourly confirms. The Edinburgh Review for March, 1819 (pp. 421, 422), with all its caution in giving an opinion on questionable data, thinks it probable that contagion *"* may be conveyed into the stomach by the saliva; or it may be absorbed by the skin *in some instances;* but *we are convinced that by far the most ordinary way is inhalation by the lungs."* That the skin does not possess sufficient powers of absorption, or the means of secreting pestilential virus, is proved by the circumstance that all animal poisons (not excepting that of snakes or the saliva of mad dogs), being innocuous, by mere contact with the skin alone ; of course, I except those cutaneous and other disorders which are not communicable by any other means. Admitting, therefore, and it is not too much, that the elements of contagion, as de l'Isle proves, are disseminated in conjunction with aqueous vapours, the efficacy of any measure for its prevention must depend upon the resistance it opposes to the passage of humidity. Now, how much soever the editor of the Edinburgh Medical and Surgical Journal may question the capability of gauze veils as preventives, Mr. Murray's communication (for which I feel much indebted to that gentleman), in your number for September, most satisfactorily demonstrates that they actually will resist the passage of moisture. An expe-

riment which I lately tried myself also confirms this. Over a given surface of boiling water I suspended two pieces of sea-weed, each weighing *four grains*, and measuring an inch square; one piece was enclosed in a single envelope of *gauze*; the other merely attached to a thread at the same elevation. After the lapse of 15 minutes, the latter was found to have gained *a grain* in weight, while the other scarcely exhibited a sensible differ-ence! I also submitted two thermometers (the one surrounded by gauze, and the other not) to different degrees of atmospheric temperature; as well as over cold water, in a warm room, when a considerable evaporation must have been going on; but I always found a variation to exist between the two. Hence, as it is well known that heat and moisture are the parents of putrefac-tion, it is more than probable that they are the generators of those epidemics which exist with greater or less virulence, in every quarter of the globe. But to recur to the remarks of the Edin-burgh Medical and Physical Journal. " Mr. B." says the writer (alluding to the inference which I had deduced from data, he conceives, " exceedingly loose and inconclusive; not to apply a harsher term") "is so fond of remote analogies, that we wonder he did not hit upon the fact of gauze thrown over fruit-trees being a protection to the tender blossoms from the hoar-frost of the mornings and evenings." I beg to assure the learned gentle-man who has so kindly furnished me with the argument, that it had not escaped my observation; and currants are to be seen at the present moment in a gentleman's garden in this place as fresh beneath a common *net* (which was the only thing made use of to preserve them from " the winds of heaven," which might otherwise have " visited them too roughly ") as they were when in season; while those exposed to the same atmosphere, with-out that protection, were withered up months ago. Want of faith in the efficacy of any measure must be respected from the regard due to opinion; but what can be said for the consistency of those who can condemn a measure as " hypothetical," as founded on analogies " loose and inconclusive," and yet recom-mend it! Yet such is the case, for " notwithstanding," says the work alluded to, " our declared want of faith in its efficacy, we are desirous that Mr. Bartlett's suggestion should be tried." The case of my little girl, which I noticed in a subsequent letter, and which appeared in your number for July, is considered even by this fastidious critic as worthy of remark. To that instance I have to add a second, wherein I subjected the same child (having taken a similar precaution) to *actual contact* with another child which had a full eruption of the measles on it at the time without any ill effects resulting from it.

I cannot close this article without referring to another profes-sional work, the London Medical Repository for July, wherein the learned editors (Drs Uwins, Palmer, and Gray,) thus speak

of my suggestion : "We consider this idea might be adopted with propriety, and *probably with effect.*"

I must confess, without presuming too much, that I look upon the favourable opinions of such men as I have named as no common sanction to the measure I have proposed; and I have only once more to express a hope that it will undergo the test of experiment upon a scale that will either confirm its utility, or disprove its efficacy.   I have the honour to be, Sir,

Your very obedient servant,

J. M. BARTLETT.

ARTICLE III.

Researches on a new Mineral Body found in the Sulphur extracted from Pyrites at Fahlun. By J. Berzelius.

(*Concluded from* vol. xiv. p. 427.)

10. *Combination of Seleniuret of Hydrogen with the Bases of the Hydroseleniurets.*

THE seleniuret of hydrogen possesses the same properties as the sulphuret and telluret of hydrogen ; it changes the blue colour of litmus to red ; unites as an acid with those bases whose radicals have a stronger affinity for oxygen than that of hydrogen ; and reduces the others to the state of metallic seleniurets. The soluble hydroseleniurets have the taste, and likewise to a certain degree the smell, of liver of sulphur, and cannot by these characters be distinguished from the hydrosulphurets; but they have a deep red or orange colour, and in this respect approach the hydrotellurets.   But I must observe that the colour of these last is much more beautiful, and has a tinge of purple, like red wine ; while that of the hydroseleniurets has a tint of yellow, and approaches the colour of strong ale.   The hydroseleniurets produce spots upon the skin, which are black, brown, or yellow, according to the intensity of the solution, and which cannot be removed by water.

I cannot determine whether this colour belongs to the hydroseleniurets as essentially as it does to the hydrotellurets, or whether it be owing to an excess of selenium dissolved by the hydroseleniuret. I melted selenium with an excess of potassium; the combination was attended with an explosion by which a great portion of the mass was lost ; but what remained gave an orange-red colour, though there was a disengagement of hydrogen during its solution. This observation seems to prove that at least the alkaline hydroseleniurets are coloured. But I ought to observe that I did not repeat this experiment, though it in some measure failed.

An experiment made with lime seems to prove the contrary of that made with potassium. I passed in a suitable vessel a current of seleniuretted hydrogen gas through a portion of lime water, taking care to exclude the atmospheric air completely. The liquid became at first muddy, and deposited a small quantity of red powder. The clear liquid remained colourless, though a great excess of seleniuretted hydrogen gas was passed through it. The first precipitate was merely seleniuret of lime, whose hydrogen had been oxidized by the air contained in the lime water. The colourless liquid left in a phial, which I considered as hermetically sealed, began, after two days, to become red at the surface without any precipitation, and the colour increased till it spread over the whole liquid. This effect being at an end, a brownish pellicle of seleniuret of lime began to form on the liquid, and small crystals of the same substance swam against the sides of the phial. After three weeks, the liquid had again become colourless. The explanation of this phenomenon is easy. The hydroseleniuret of lime in contact with the air is decomposed; but the resulting seleniuret remained combined with the undecomposed part, constituting a seleniated hydroseleniuret. This combination having at last reached its point of saturation, the decomposition of the hydroseleniuret occasioned the precipitation of seleniuret of lime, partly in the form of a pellicle, and partly in crystals. The colourless liquid from which this last was deposited contained still lime in solution. This proves that seleniuretted hydrogen can neutralize a greater quantity of base than selenium alone. The same is the case with sulphur.

The best method of producing the hydroseleniurets is to dissolve seleniuret of iron in muriatic acid, and to make the gas pass into a Woulfe's apparatus, in which the gases are dissolved, or mixed with water. Seleniuretted hydrogen gas is absorbed much more rapidly and completely than sulphuretted hydrogen gas, and we do not lose so much of it as of this last.

The hydroseleniurets are decomposed by the contact of air, and the selenium is deposited pure from those which have an alkali for a base. It is generally deposited at the surface in the form of a pellicle, which, on the upper surface, is smooth, metalline, and of the colour of lead. The under surface has a deeper grey colour, and a crystallized texture. If the decomposition takes place slowly, without agitation, in a vessel which has more depth than breadth, the selenium is deposited in crystalline vegetations on the part of the glass turned towards the light.

The danger with which I considered experiments with seleniuretted hydrogen gas to be attended prevented me from obtaining the hydroseleniurets in a dry state. The only base with which I have made an experiment is ammonia; but it did not succeed to the degree that I expected. Into a small glass filled with mercury, I passed dry ammoniacal gas till the vessel

was half filled with it. I then introduced seleniuretted hydrogen
gas which had passed through a tube filled with anhydrous
muriate of lime. The two gases formed a white smoke, which
was soon deposited both on the mercury and the glass, and
formed a mass of a pale-red colour, in which no traces of crys-
tallization could be observed by the microscope. I cannot decide
whether the red colour depends upont he presence of a trace of
air mixed with the ammoniacal gas ; but that circumstance is at
least possible. The fixed salt, when dissolved in water, gave
that liquid a deep-red colour.

Barytes, strontian, lime, and magnesia, all form soluble hydro-
seleniurets. The hydrate of magnesia mixed with water, through
which a current of seleniuretted hydrogen gas is passed, is
dissolved easily by means of an excess of the gas. The solutions
of the other earths mixed with hydroseleniuret of ammonia form
flesh-coloured precipitates, with the exception of alumina, which
gives a precipitate of a deep-red colour. As the liquid retains
no trace of seleniuretted hydrogen, it is to be presumed that
these precipitates are real hydroseleniurets. All the metallic
solutions are precipitated by the alkaline hydroseleniurets. The
precipitates formed in the salts of zinc, manganese, cerium, and
probably likewise of uranium, are hydroseleniurets. They are
speedily decomposed by the access of air, and their pale-red
colour becomes at the same time much deeper. The salts of the
other metals are reduced to metallic seleniurets, and the precipi-
tates which they produce are black or dark-brown, and assume
the metallic lustre when strongly pressed by a polished hema-
tites.

Before quitting this subject, I must be allowed to make an
observation relative to the nomenclature of these combinations.
Chemists have begun in the English and French nomenclatures
to call the combinations of sulphuretted hydrogen with bases
hydrosulphates, to indicate the same theoretical relation between
sulphuretted hydrogen and sulphuric acid, which, in the new
hypothesis, respecting the nature of oxymuriatic gas, is consi-
dered as established between hydrochloric (muriatic) acid and
chloric acid. But without entering into any discussion respect-
ing this last hypothesis, I think I may state that the new names
given to sulphuretted hydrogen gas and its combinations with
bases are contrary to the spirit of the nomenclature ; since, when
hydrogen is taken away from hydrosulphuric acid and from the
hydrosulphates, the residuum is not sulphuric acid and sulphates,
but sulphur and sulphurets. I think, therefore, that the old
name expresses the nature of the substance which it is intended
to indicate much better than the new ; and that this change of
nomenclature has been made without sufficient reason. This is
the reason why in this memoir I have not adopted the new
names, either for the hydrosulphurets, hydroseleniurets, hydro-
tellurets, or for a variety of other substances ; and I think that

we cannot be too much on our guard against adopting and propagating changes, which are not necessary in a nomenclature once admitted, since long custom renders it difficult to correct even a bad nomenclature. It will be, perhaps, objected to me that sulphuretted, seleniuretted, and telluretted hydrogens actually possess some of the principal characters of acids. I allow this. But what confusion in the nomenclature would be produced if we thought proper to call every substance an *acid* which in a combination may act the part of an electro-negative body ! Do we call every substance an alkali which is capable of acting the part of an electro-positive body ; that is to say, a base ?

11. *General Observations relative to the Properties of Selenium.*

The existence of a body whose properties constitute, so to speak, the transition from the non-metallic combustible bodies to the metal is surely a very interesting phenomenon ; while this body possesses some of the characteristic properties of the metals, for example, the metallic lustre ; it is destitute of others equally essential, for example, of the power to conduct electricity and heat. In fact, as there is no positive line of separation between the chemical properties of these two classes of bodies, it is probable that none ought to exist between their external properties.

We have seen that selenium is more analogous to sulphur than to any other body, and next to sulphur, it resembles tellurium; so that it lies between these two bodies. But it is from its properties that we must determine in which of the classes we must place it, whether among the metals, or along with sulphur, phosphorus, boron; that is to say, the class of substances to which I have ventured to give the name of *metalloids*. In itself it is indifferent in which of the two classes we place it, since the limits between them are not determinable, and since selenium possesses to such a degree the characters of both, that we may place it with equal justice in either of the two. The metallic lustre and the specific gravity have been considered as the characteristics of the metals ; but the last of these has ceased to be a character, because we are now acquainted with metals which are lighter than water. Nothing remains but the lustre ; and I believe that if sulphur and phosphorus possessed the metallic lustre, nobody would hesitate to place them among the metals. As selenium possesses this property in a high degree, and as it must of necessity be placed in one of the two classes, I conceive that we may arrange it in preference among the electro-negative metals, that is to say, among those which produce acids. It will begin the series in making the transition from sulphur and phosphorus to arsenic.

The number of simple combustible bodies has been lately augmented by three ; two of which with equal, if not with greater

reason, may be considered as oxides, which it has been hitherto impossible to reduce ; while the third has a problematic existence. These bodies are chlorine, iodine, and fluorine. To render the simplicity of these bodies more probable, advantage has been taken of their supposed analogy with sulphur and phosphorus. It is evident that in proportion as more bodies are discovered and compared with these last, this analogy must either increase or diminish in probability. This is the reason that induces me to compare selenium with sulphur, chlorine, iodine, and some other bodies.

Sulphur and selenium, on one side, unite with the metals ; and these combinations, which, for the most part, still preserve some of the external properties of metals, retain likewise the combustibility of their radicals. The sulphuret and seleniuret of potassium and sodium are soluble in water. They decompose it at the same time, and produce hydrosulphurets and hydroseleniurets.

Chlorine and iodine, on the other side, combine likewise with the metals ; but their combinations have generally the same characters as the anhydrous sulphates, phosphates, and arseniates. The radicals, at least the most combustible, have lost their combustibility : the chloride and iodide of potassium and sodium do not unite with oxygen ; do not decompose water ; and do not form hydrochlorates and hydriodates (unless it be pretended that the chlorides of potassium and sodium decompose water with the production of cold ; while the chlorides of calcium and barium decompose it and produce heat). On the contrary, the chlorides of copper, tin, and gold (and of several other metals, which are incapable of decomposing water, either alone, or in the state of sulphurets, or mixed with the strong acids), decompose water, and produce hydrochlorates. There is, therefore, no analogy between sulphur and selenium on the one side, and chlorine and iodine on the other, since they produce entirely opposite phenomena.

Sulphur, selenium, and tellurium, combine with hydrogen, and produce particular acids, which are gaseous and very weak. It had been long observed that sulphur and tellurium, notwithstanding the difference in their physical and chemical properties, form combinations with hydrogen of characters astonishingly analogous. We now find a third body with which hydrogen forms a compound, having the same smell, the same taste, and the same properties, as the two preceding ones. This class of acid bodies gives with all the oxides, whose radicals have a stronger affinity for oxygen than hydrogen has, a particular kind of salt, which preserves the taste, and in part even the odour of the acids ; though they are less capable of neutralizing the bases than other acids. The oxides, whose radicals have a weaker affinity for oxygen than that of hydrogen, are decomposed by these acids. The result is water, and a combination of the radi-

cals.   These facts lead us to presume that the hepatic taste and
smell, from being peculiar to sulphur, are common to that class
of acid bodies, as well as to their combinations with bases ; and
that an hepatic taste and smell are as essential to the hydracids
and to their combinations with alkalies, as the acid taste and
smell are to the strong oxacids, and the saline taste to their neu-
tral combinations with alkalies.   I, therefore, think it very
probable that an alkaline salt destitute of the hepatic taste does
not contain an hydracid ; and, on the contrary, if the taste be
saline, that it contains an oxacid.   By extending these consider-
ations still further, we find that the oxacids, whose names termi-
nate in *ous*, give to their alkaline salts a peculiar taste by which
we recognise the acid ; for example, the sulphites and the phos-
phites.   Further, another class of oxacids very weak, and very
indistinctly acid, as the tungstic and antimonic acids, the oxide
of tellurium, &c. give to their compounds with the alkalies a
metallic taste ; so that each class appears to have general
common properties by which the class can in some measure be
known.

Chlorine and iodine combine likewise with hydrogen ; but
these combinations are very strong acids, and have their taste
and smell purely acid.   These acids have the peculiar property
of being able to reduce, by means of their hydrogen, potash and
soda, and of forming chlorides and iodides, whose pure saline
taste is entirely analogous with the combination of the strong
oxacids with the alkalies.   On the other hand, the hydrogen of
these acids does not reduce the oxides of copper, bismuth, gold,
&c. with which the hydrochloric and hydriodic acids combine
without decomposition.   Thus we perceive that the analogy of
these hydracids with those of which we have just spoken is
entirely null.   I thought it proper not to omit these comparisons,
because they constitute an addition to the circumstances, which
sooner or later will give us more satisfactory information respect-
ing the nature of muriatic, iodic, and fluoric acids.

We have seen the great analogy between sulphur and selenium,
and this analogy continues even in their combinations with
oxygen; so that both form acids but little volatile.   However,
these acids have not the same analogy with each other as their
radicals have.   Sulphuric acid belongs to that numerous class of
acids which have three atoms of oxygen, and possesses in con-
sequence the manner of combination of these acids.   Selenic
acid, on the contrary, belongs to the small class of acids which,
without terminating in *ous*, contain only two atoms of oxygen;
and in this respect has a strong analogy with carbonic and
boracic acids.   Like these acids, it does not form neutral salts
with alkalies.   In the salt, in which the acid contains twice as
much oxygen as the base, the alkali re-acts ; and in those in
which the acid contains four times as much oxygen as the base,
it is the acid which re-acts.   We find the same phenomenon in

the borates and the carbonates; yet boron and carbon have no analogy with selenium. It is true that boron may be combined with the alkalies; but I do not know that any one has hitherto produced a boruret of hydrogen or borurets analogous to the metallic sulphurets and seleniurets. Every one knows that carbon does not combine with the alkalies, and in a very small proportion with the metals; and, finally, that carburetted hydrogen has not the hepatic taste and smell, nor possesses the properties of an acid.

Selenium has no analogy, as far as its chemical characters are concerned, with arsenic and phosphorus. It is obedient to the general laws of the combination of oxidated bodies, from which the two former deviate in so remarkable a degree. Their combinations with hydrogen, though possessing several properties of the gases containing much hydrogen, are not hepatic, and do not possess the properties of acids. We may say that arsenic is to phosphorus what selenium is to sulphur; but in fact there is very little analogy between these two pair of bodies.

12. *Researches into the State in which Selenium is found in the Mineral Kingdom.*

By the care of my friend M. Gahn, I received, during my experiments, a portion both of the pyrites of Fahlun, employed in the manufacture of the sulphur, and of the impure sulphur itself.

The pyrites was partly pure and partly mixed with blende, galena, chlorite, and several other foreign bodies. By roasting either before the blow-pipe or on a furnace, it was impossible to perceive any smell of horseradish.

I dissolved 10 grammes of this pyrites in nitromuriatic acid. The solution was precipitated by sulphuretted hydrogen gas, and the precipitate was again dissolved in nitromuriatic acid. The liquid was then saturated with potash, filtrated, and evaporated to dryness. The salt obtained was mixed with sal-ammoniac, and exposed to a high temperature. After having dissolved the saline mass, selenium remained; but its quantity was so small that it could not be weighed.

The impure sulphur gave, by a similar treatment, about 0·0015 of its weight of selenium. This quantity, small as it is, would furnish a considerable annual quantity of selenium, did not the other impurities, and particularly the arsenic, hinder it from being employed. The purified sulphur furnished traces of selenium scarcely perceptible.

The experiments with the pyrites of Fahlun appeared to show that the selenium is scattered through the whole substance of the stone, although in a quantity infinitely small. However, as it sometimes happens at Fahlun that the roasting of the copper pyrites exhales a strong odour of horseradish, we may presume *that a mineral* containing selenium occurs here and there in a

more notable quantity ; and we must hope that it will one day be in our power to recognize and collect it.

M. Gahn had shown me some years ago a small piece of a mineral which he had received under the name of *Swedish ore of tellurium*. I made some fruitless attempts to extract tellurium from it, and at the time that I thought myself entitled to say that it contained none of that metal, M. Gahn made me perceive the strong odour of horseradish which it exhaled when heated before the blow-pipe. The small quantity which we possessed being consumed in our attempts to obtain tellurium, I was obliged to defer the examination of it till I could procure a greater quantity of it.

During my experiments on selenium, I recollected this pre- tended ore of tellurium, and having applied to the person who had sent the specimen to Gahn, I was fortunate enough to obtain a quantity sufficient for an analysis of it.

As far as I can judge from the specimens of this mineral which I have seen, it possesses the following properties :

Its colour is leaden grey ; it has the metallic lustre ; the frac- ture is granular and subcrystalline, without its being possible to discover other signs of crystallization. It is soft, and may be cut with a knife ; where cut, it has the brilliancy of silver. It receives impressions from the hammer.

Before the blow-pipe, it melts, and gives out a strong smell of horseradish, leaving a small grey metallic button, which conti- nues long to exhale the smell. If we fuse it with borax, that saline substance becomes copper-green, and a brittle metallic button separates, which is a seleniuret of silver. A solution of this mineral in boiling nitric acid mixed with cold water gives a white precipitate, which is seleniate of silver, and which has probably led to the notion that it was an ore of tellurium.

The mineral is mixed with carbonate of lime and with black parts, which, when scratched by the knife, assume the metallic lustre, melt with difficulty before the blow-pipe, giving out the odour of selenium, dissolve, when fused with borax, giving it the green colour of copper, and exhibit no traces of reduced silver. The black parts appear to be a serpentine imbibed with seleniuret of copper.

For analysis I selected pieces as pure as possible, and I divided them into very small grains, to be sure that they con- tained no visible portion of these foreign bodies.

a. One hundred parts of the mineral were dissolved in boiling nitric acid. The solution was diluted with boiling water, and then filtered. The liquid which passed fell into a solution of muriate of soda, and the matter which remained upon the filter was washed with dilute boiling nitric acid as long as the liquid that passed through continued to trouble the solution of the muriate.

In this last solution, muriate of silver had precipitated, which,

after being well washed, dried, and fused, weighed 50·7 parts, equivalent to 38·93 parts of metallic silver. On the filter remained a mixture of silica and stony matter foreign to the composition of the dissolved mineral. When heated to redness, it weighed four parts.

b. The liquid from which the silver had been separated was precipitated by sulphuretted hydrogen gas. The precipitate was redissolved in nitromuriatic acid, and the acid liquid concentrated till the whole nitric acid was decomposed. It was then diluted with water, and sulphite of ammonia added. It became gradually muddy, and of a cinnabar-red colour. After some hours, it was boiled, adding from time to time small portions of sulphite of ammonia. The boiling was continued two hours to be quite sure of precipitating the whole of the selenium. Collected on a filter, dried, and almost fused on the filter, it weighed 26 parts.

c. The liquid separated from the selenium and deprived by boiling of the residual sulphurous acid, was precipitated by the subcarbonate of potash. The green precipitate being washed, dried, and heated to redness, became black, and was peroxide of copper. It weighed 27 parts, equivalent to 21·55 parts of metallic copper. This oxide, dissolved in muriatic acid, and mixed with an excess of ammonia, dissolved completely with the blue colour of copper. The alkaline liquid from which the carbonate of copper had been separated, had still a green tinge. It was concentrated, acidulated with muriatic acid, and, by means of an iron plate, 1·5 part of copper was still separated from it, which makes the whole of the copper to amount to 23·05.

The liquid precipitated in *b* by sulphuretted hydrogen gas, being deprived by boiling of the excess of gas, was mixed with caustic ammonia, which occasioned a yellow precipitate, which, when dried, weighed 1·8 part. It was a mixture of oxide of iron and alumina. The liquid from which it had been separated was mixed with an excess of subcarbonate of potash, and evaporated to dryness. The saline mass, being dissolved in water, left a white earth, which, when dried, weighed 3·1 parts. Sulphuric acid, being mixed with this earth, occasioned an effervescence with the production of gas. The liquid separated from this last and evaporated became gelatinous, and deposited silica. It appeared also to contain magnesia ; but I neglected it because these earths were obviously foreign to the metallic mineral.

The result of the analysis was :

Silver . . . . 38·93
Copper . . . . 23·05
Selenium . . . . 26·00
Foreign earthy matter . . . . . . 8·90
Loss . . . . 3·12
                                          ———
                                          100·00

This great loss must be partly attributed to the carbonic acid united to the lime, in a great measure to the selenium which it is difficult to separate completely, and partly to the unavoidable loss in this kind of experiment.

The 38·93 parts of silver combine with 2·86 oxygen. The 23·05 of copper require to form the protoxide 2·91 parts of oxygen; and the 26 parts of selenium in order to be acidified require 10·5 parts. We see from this that the two metals absorb an equal quantity of oxygen, and that the selenium absorbs twice as much; that is to say, that they are combined in the same proportion as in the neutral seleniate of silver and the protoseleniate of copper. Hence the chemical composition of this mineral may be expressed by the following symbol; 2 Cu Se + Ag Se². It may be requisite to recall to the attention of the reader another mineral recently described by MM. Haussman and Stromeyer,* to which they have given the name of *silber-kupfer glans*, and the composition of which, according to their analysis, is 2 Cu S + Ag S²; so that there exists native a double sulphuret analogous to the double seleniuret, which I have just described. These two minerals, therefore, ought to be placed beside each other in the mineralogical system.

As this mineral requires a name not derived from its composition, for the sake of shortness, I have called it eukairite (from ευκαιρος, *opportunus*); because I consider the accidental discovery of selenium in the mineral kingdom just at the time that I had finished my experiments on this interesting body, as a peculiarly fortunate circumstance.

I could not at first discover whence this pretended ore of tellurium came; but having consulted M. Hisinger, to whom Swedish mineralogy lies under such obligations, he immediately knew that the mineral came from an old abandoned copper mine at Skrickerum, in the parish of Tryserum, in Smoland, and that specimens of it were occasionally observed in old collections under the name of native bismuth of Skrickerum. I examined afterwards the specimens from that mine found in the collection of the College of Mines at Stockholm, and had the satisfaction to find a very good specimen of eukairite. It is surrounded by a serpentine of a black or dark-green colour, which, at the point of contact with the eukairite, is penetrated by seleniuret of copper, in which only traces of silver can be observed. This seleniceferous serpentine is found here and there in the mass of the eukairite, as I remarked above. The quantity of seleniuret of copper in the serpentine diminishes in proportion as its distance from the eukairite increases. In its neighbourhood, it becomes metallic when rubbed with a hard body; but this does not happen at a distance from the eukairite; and when the interval amounts to a line and a half, all trace of selenium disappears.

* Gilbert's Annalen der Physik, 1816, vol. x. p. 111.

This serpentine, when treated with concentrated muriatic acid, is decomposed; but the liquid contains neither selenium nor copper: these two bodies remain undissolved with the silica—a proof that neither of them existed in the mineral in the state of oxide. On the other side, both nitric and nitromuriatic acids dissolve the two metals before they have begun to attack the serpentine. In the mine of Skrickerum, we find the following minerals along with the eukairite: yellow sulphuretted copper, sometimes iridescent, sometimes in its natural state, and sometimes with a compact and tarnished fracture; carbonate of lime; serpentine partly black, partly dark-green, and partly pale-green; and anthracite mixed with carbonate of lime in very thin beds, which intercept the anthracite at the distance of one-sixth, or one-third of a line.

The discovery of selenium at Skrickerum occasioned a more attentive examination of what might be found from that mine in collections. M. Swedenstierna found beautiful specimens of a carbonate of lime, which had great black spots in it, and he sent me a specimen of it for examination. I observed that these spots were formed round natural clefts in the carbonate, and when the mineral was divided through these clefts, the new surfaces were found covered with a white metallic vegetation. A small plate of the black mass surrounding the clefts, being seen through a compound microscope, presented a metallic vegetation, which penetrates the substance of the limestone in every direction.

I separated the metallic substance from the lime by dissolving this last in muriatic acid. A considerable black mass remained, which was dissolved in nitric acid. The solution, being mixed with nitrate of barytes, did not give any precipitate. Muriate of soda precipitated nothing; but after an interval of some minutes, the liquid began to assume a milky aspect, and in some hours gave traces of a precipitate. The liquid, which was blue, being mixed with an excess of ammonia, did not let any thing fall; but with subcarbonate of potash, it formed subcarbonate of copper, and I then separated from the residual liquid, rendered acidulous by muriatic acid, selenium, by means of sulphite of ammonia. Hence the mineral which produces these black stains is a seleniuret of copper without any mixture of silver. Likewise the metallic vegetation found in the clefts contains no traces of silver, as I ascertained by detaching a small quantity which had been dissolved in nitric acid and then mixed with muriatic acid, without any other result than a feeble opalescence.

It is well worth remarking that it is only the seleniuret of copper which is found filtered into the porous parts of the carbonate of lime, as well as of the serpentine. Hence it seems to follow that the seleniuret of copper was more liquid than the eukairite at the time when these stony masses assumed their actual form.

Seleniuret of copper has a paler colour than eukairite, and has almost the appearance of native silver. It is soft, may be hammered and polished, and then assumes the colour of tin. Even the black spots of the carbonate of lime assume a metallic polish when filed or rubbed against a hard body. When heated in the requisite vessel, it does not yield selenium. Hence it is a protoseleniuret.

We have then two species of minerals containing selenium; both of which in the chemical system of mineralogy belong to the family of copper; namely, *seleniuret of copper* = Cu Se, to which I think it superfluous to give any other name than the chemical appellation; and *eukairite* = 2 Cu Se + Ag Se².

Article IV.

Answer to Mr. Venables's Queries respecting Cyder-making.
By Chemico-Medicus.

(To Dr. Thomson.)

SIR,   *Bolton-row, Piccadilly, Oct.* 9, 1819.

The attention I have paid to the making of cyder, and other vinous liquors, in this country, has emboldened me to answer the queries of your correspondent, the Rev. Mr. Venables, relative to fermentation of the juice of the apple.

To the first Query.—I know of no method of neutralizing the malic acid, except by an alkali, which, of course, would injure the cyder. If the apples which yielded the juice were not ripe, it certainly would be advisable to add to it, previously to fermenting it, a quantity of sugar, otherwise the fermented liquor will be so weak that it will soon run into the acetous fermentation. If this addition be not made, a portion of some alcohol, as brandy, rum, or malt spirit, should be added, to keep the cyder in a vinous state. Brandy, I know, is made use of by some cyder-makers, near Ledbury, in Herefordshire, with a view of preventing it passing beyond the vinous stage, or, as they say, to preserve its richness; and those makers are particularly celebrated for making superior cyder. The addition of a spirit, after the first stage of fermentation, I consider to have the same effect as that of sugar previous to fermentation; the advantage of the latter arising from a production of a spirit which preserves the cyder in a vinous state. Another circumstance which powerfully contributes to this end is the grinding of the kernels of the apples, as suggested, I think, by Mr. R. Paine Knight, the *peculiar bitter quality* of which not only gives the cyder a fine flavour, but very powerfully tends to preserve it in a vinous state. Some months ago I was led to make experiments with the saccharine

roots of this country by grinding them with apples, in consequence of seeing in a periodical work, entitled The Gazette of Health, the beet-root recommended to be added to the apples at the time of grinding, with the view of imparting saccharine matter. The cyder ·was· certainly much enriched by the addition, but I was not a little disappointed in the colour, which I expected to have found beautifully red. The colour imparted to the recently expressed juice by the beet-root was entirely destroyed by the fermentative process. The cyder had a peculiar earthy flavour, which was not relished by cyder drinkers. Both the parsnip and the carrot considerably enriched the juice of the apples, and did not affect the flavour of the cyder. It is worthy of notice that in Herefordshire and in Devonshire the farmers add a considerable quantity of water to the apples at the time of grinding them, which they contend renders it rough. The water is generally in such quantity as to constitute more than one-half of the expressed liquor. This is the cyder generally drunk in both counties, and preferred by the farmers for their own use. Hearing a farmer, who made about 800 hogsheads annually, obstinately contend that an addition of water to the apples, when in the mill, increased the strength of the cyder, I was induced to examine the article thus made, and also that which was not *strengthened* by adding water. The former I found did not afford half the quantity of spirit as the latter. To the palate the former, as the farmer contended, appeared stronger and rougher. On examining it, I soon discovered that this supposed strength depended on the presence of vinegar! The fact was soon evident, that the expressed juice thus diluted speedily ran into the acetous fermentation, and that instead of cyder, they were drinking a dilute vinegar; and to it the natives give a preference. Indeed cyder in a proper vinous state, they condemn, supposing, on account of its being somewhat sweet, that it has been " *doctor'd.*"

Second Query.—The effect of boiling the *malic acid,* as it exists in cyder, will be a dissipation of the spirit, and consequently the liquid will soon become vinegar. Your correspondent's query is by no means clear to me—I presume he means the boiling of cyder, and not the *pure* (concentrated) *malic acid,* which he must be aware can undergo no change by boiling.

Third Query.—I know of no other means of clearing the liquor before fermentation from impurities than by straining it. During fermentation much passes off through the bung-hole, and much is deposited. If it remains foul after fermentation, it may be cleared by isinglass.

Fourth Query.—I cannot speak positively as to *close* fermentation. Mr. R. Paine Knight recommends *close* fermentation, by which, he says, the flavour of the apple is preserved. I have tasted very fine cyder that was thus fermented, but a quantity of brandy was added to preserve it in a vinous state. If the fermentation be conducted in a wide vessel *entirely* open, the alcohol

will fly off, and the liquor will rapidly advance to the acetous fermentation. I think it right, however, to allow the carbonic acid gas to pass off.

Fifth Query.—The lees of cyder is only the feculent part of the liquor deposited. I should suppose that no person could expect such an article to contain as much spirit as clear cyder.

Sixth Query.—That cyder is weakened by racking is very evident, because the spirit flies off. This process is often necessary to *quiet* cyder, by getting rid of carbonic acid gas. After parting with this gas and a portion of alcohol, the feculent matter diffused through it is generally deposited.

I see no reason why cyder should not be made in this country equal to many of the Rhenish wines. The juice of the apple contains a sufficient portion of acid, and the malic acid is assuredly as pleasant as that of any grape. The expressed juice is deficient in saccharine matter, and this deficiency may be made up by adding that of other vegetables. For this purpose, I prefer germinated wheat. By grinding this article with the apples, the juice is considerably enriched; and, when properly fermented for three days with a little yeast (about half a pint to 120 gallons), the cyder will be found equal to the wine commonly drank in Germany.

In many parts of Herefordshire and Worcestershire, the crab is very abundant. The fruit the farmers seldom collect; and when they do, they grind it for making verjuice, which is sometimes used for vinegar, but generally kept for bruises and sprains. This acid differs only from that of the apple in strength. If the juice be, therefore, diluted with water, and a quantity of sugar added (in the proportion of an ounce to a pint), the fermented liquor will be equal to the best cyder. I have known this liquor pronounced *excellent* cyder by good judges of the article. There is in the cyder counties a strange prejudice against the employment of sugar in making of cyder. The vulgar suppose that the article is rendered weak by it, and that the only object for which it can be used is to render it palatable. Such cyder, they say, is only fit for ladies.

If you deem this communication worthy a place in your journal, I shall occasionally contribute my mite towards its laudable object.   I am, Sir, your obedient servant,

           Chemico-Medicus.

On reperusing the foregoing letter, I find I have omitted to notice one remark made by Mr. Venables respecting the exposure of the pulp of the apple to the atmosphere. This is particularly recommended by Mr. R. P. Knight, who asserts that the juice thereby acquires an increase of saccharine matter. The experiments I have made certainly confirm this statement; but cyder so made, I have thought, sooner runs into the acetous fermentation, probably in consequence of attracting oxygen.

ARTICLE V.

On the Cornea of the Eye. By Mr. Booth.

(To Dr. Thomson.)

SIR,   *Barnet, Nov.* 20, 1819.

As the pages of your *Annals* are not generally occupied by anatomical subjects, it might be supposed the following observations were misplaced. But as they refer to the structure and functions of the eye, and a peculiar action of that organ upon light; they are so connected, perhaps, with the other more immediate objects of your journal as to allow of their insertion.

It is a well-known anatomical fact, that if a moderate degree of pressure be applied to the sides of an eye after removal from the socket, its axis becomes increased; the crystalline lens is thrust forward; the cornea becomes distended by means of the aqueous humour; and, at the same time, is rendered perfectly opaque. This opacity is not permanent; for the moment the pressure is removed, it regains its former transparency, and this effect may be continually varied as we vary the pressure. I was induced to examine this phenomenon rather more minutely, from having lately observed that it was not produced in very fresh eyes; and hearing it remarked that if all the layers of the cornea, except the last, are removed, this effect does not take place.

I had originally considered this opacity to depend upon a peculiar polarity, given by means of the pressure to the aqueous humour contained in the anterior chamber of the eye, and which might become more obvious a short time after death on account of some change that might have taken place in the nature of the fluid. This, however, could not be the case, as the opacity ceases on the removal of the several layers of the cornea, although an equal degree of pressure be applied. It must, therefore, evidently depend upon some change produced by the pressure of the aqueous humour upon that membrane. The cornea cannot, in this instance, be in its natural state; and some mechanical or morbid alteration must previously have taken place before the opacity could have been produced. I removed several corneæ from eyes and placed them between slips of glass; upon applying pressure, those which had been taken from recent eyes suffered no change; while the others became opaque. The opacity was the same by transmitted as by reflected light. This rendered it certain as to its depending upon the cornea, and to some change that must have taken place in it. After death, a transudation of the aqueous humour takes place through the layers of the cornea, and it is to the quantity of this fluid contained between the layers that the opacity of the cornea may be referred. Whether the opacity depends upon pressure com-

municated by the layers of the cornea to the particles of aqueous fluid contained between them; or by a mutual action upon each other, I am at present unable to say. But the fact is evident that if pressure be applied to a cornea containing aqueous humour between its layers, a peculiar polar arrangement takes place producing perfect opacity. I should not suppose any change to have taken place in the nature of the cornea; for even in the fresh eye, the effect may be produced, by applying a remitted pressure for a short time—pumping, as it were, the fluid into the layers of the cornea. If the tunica conjunctiva be removed, the fluid will be seen exuding from the surface in the form of dew, and the effect gradually takes place. It is more obvious when the conjunctiva is not removed, on account of its being a more impervious membrane, and by offering a resistance to the passage of the humour allows a greater accumulation of it between the layers of the cornea. When all the layers are removed except the last, the whole structure upon which the phenomenon depended becomes destroyed; for in this case, the fluid having only one layer to pass through, it escapes without any degree of pressure, and without producing any effect; whereas when more than one layer exist, the fluid becomes entangled between them, and in this situation is affected by the pressure from behind.

In Mr. Wardrop's Anatomy of the Eye, he mentions a case of opacity resulting from a larger secretion of aqueous fluid than usual; and being induced to puncture the cornea to allow some of it to escape, he found the transparency instantly restored. The opacity, in this instance, most likely depended upon some morbid peculiarity of the cornea, communicated to it by the disease existing in the contiguous membrane of the aqueous humour, and which had primarily produced an increase of that fluid. This morbid peculiarity allowing of a transudation of the fluid between its coats, which, being acted upon by the pressure from the increased quantity of aqueous fluid, produced the opacity. This case would appear to depend more upon the morbid state of the cornea than the pressure; for we have often an increase in the quantity of aqueous fluid in the anterior chamber of the eye, producing pressure, but not opacity.

By causing various fluids to pass between the layers of the membrane, I was in hopes of arriving at a more certain knowledge of the nature of this polarity; my results, from the imperfect manner in which my experiments were performed, were too various to place much confidence in them. As I intend pursuing the subject, should I be able to arrive at any thing determinate, I may again obtrude myself upon the pages of your journal.

I am, Sir, your most obedient servant,

THOMAS S. BOOTH.

ARTICLE VI.

Experiments on the Gas from Coal, chiefly with a View to its Practical Application. By William Henry, M.D. F.R.S. &c.

(*Concluded from* vol. xiv. p. 344.)

On the Purification of Coal Gas.

THE chief impurities mingled with the gas from coal, which it is desirable and practicable to remove before applying it to use, are carbonic acid and sulphuretted hydrogen gases. The former is of little importance; but the latter imparts to the coal gas, when unburned, a very offensive smell, resembling that of bilge water, or the washings of a gun-barrel, and the inconvenient property of tarnishing silver plate; and during combustion gives rise to the same suffocating fumes (sulphurous acid) which are produced by the burning of a brimstone match. The most obvious method of absorbing both the carbonic acid and the sulphuretted hydrogen is to bring the recent gas into contact with quicklime; and the cheapness of that substance, and facility of applying it, led me, several years ago, to propose it for the purpose.* It has since, I believe, been suggested that the sulphuretted hydrogen may be removed by chlorine; but a sufficient objection to this agent is, that it would also separate the most valuable part of the product, the olefiant gas. The transmission of the gas through ignited tubes has also been proposed; but it is a well-known property of both the varieties of carburetted hydrogen, that they deposit charcoal, when strongly heated; and M. Berthollet has shown that the amount of this effect is proportionate to the increase of temperature.† Some persons practically engaged in lighting with gas have, to my knowledge, been led, by the increase of the quantity of gas which is obtained by passing it through red-hot tubes, to imagine that an advantage is thus gained; and they have not been aware that the gas, when thus treated, sustains a much more than proportional loss of illuminating power.

The quantity of quicklime required for the absorption of a cubic foot of carbonic acid, or of the same volume of sulphuretted hydrogen gas, will be found on calculation not to exceed 1050 grains, or about $2\frac{1}{4}$ ounces avoirdupois. A volume of coal gas containing a cubic foot of each of those impurities will require, therefore, at least five ounces of lime applied in the best possible manner. But it is never found in practice that the whole of any gas, when sparingly diffused through another, can be taken out entirely, without using much more of the appropriate agent than, from its known powers of saturation, might have been deemed equivalent to the effect. The proportion employed by Mr. Lee

* Phil. Trans. 1808, p. 303.   † Memoires de la Soc. d'Arcueil, iii. 154.

is five pounds of fresh burnt lime to 200 cubic feet of gas. The lime, after the addition of the quantity of water necessary to reduce it into powder, is passed through a sieve, and then mixed with a cubic foot (about $7\frac{1}{2}$ wine gallons) of water. This is found to be enough to purify the gas sufficiently for ordinary purposes; but it still retains a minute proportion of sulphuretted hydrogen, which, from the shade of colour produced in the test, may be estimated at about $\frac{1}{10000}$th of its volume. For some purposes, the same gas is therefore washed a second time with a similar proportion of fresh lime, which, without being removed from the cistern, is again employed to give the first washing to another quantity of fresh gas. After the second purification, the gas produces no change whatever in the test, which preserves its perfect whiteness, thereby demonstrating the complete removal of the sulphuretted hydrogen. In this state of purity, its odour also is so much diminished as scarcely to be at all offensive.

In order to ascertain whether any, and what portion of olefiant or carburetted hydrogen gas is lost by the action of the lime liquor, I compared, with the greatest care, the products of the combustion of the recently prepared gas, and of the same gas after one and two washings with lime and water.

	Consumed oxygen.	Gave carb. acid.
100 measures of the unwashed gas ..	190	108
Gas once washed	175	100
Twice washed....................	175	100

The frequent repetition of similar experiments fully satisfied me that the fresh prepared gas from coal does in fact sustain, by agitation with lime liquor, a loss of combustible matter amounting to about 8 or 10 per cent.; but that the second washing is not attended with any further appreciable loss. I found also that the recent gas, by being kept a fortnight in bottles completely filled with it, and well stopped, so as to exclude all agency of the water in which they were inverted, was diminished in combustible matter about half the foregoing amount. On the other hand, gas which had been washed with lime liquor suffered no change, when kept under like circumstances for an equal time. It is probable, therefore, that what is separated from the unwashed gas, whether by keeping or by the action of lime liquor, is chiefly condensable matter, partly perhaps an etherial oil, and partly a substance which it is desirable to remove, rather than to allow it to be deposited in a solid form, in the small pipes, or in the burners.

The little effect of the lime liquor on the olefiant gas, which I had not anticipated, admits, however, of being satisfactorily explained on known principles. Water and similar fluids absorb, according to Dalton, about one-eighth, according to Saussure, about one-seventh, of their volume of olefiant gas. The utmost

quantity, therefore, which a cubic foot of lime liquor, acting
upon pure olefiant gas, could absorb, would be one-seventh of a
cubic foot.   But agreeably to a law discovered by Mr. Dalton,
and explained and confirmed by my own experiments,[*] a cubic
foot of lime liquor, when brought into contact with 36 cubic feet
of olefiant gas mixed with 164 cubic feet of other gases, can
absorb only about one-fifth of one-seventh, or $\frac{1}{35}$th of a cubic
foot of olefiant gas.   This quantity, which does not exceed
$\frac{1}{1400}$th part of the olefiant gas present in 200 cubic feet of the
best coal gas, is too trifling a loss to be discoverable by experi-
ment, or to be worthy of being regarded in practice, even when
doubled by a second washing.   It is, therefore, consistent with
general reasoning, as well as with experiment, that the washing
of coal gas with a due proportion of lime liquor should entirely
remove the sulphuretted hydrogen gas and other offensive ingre-
dients, without abstracting an appreciable quantity of either of
the carburetted hydrogen gases.   It is nevertheless important
that the quantity of water, employed in washing the gas, should
not be increased beyond what is necessary to give the mixture
due fluidity, because, under equal circumstances, the power of
water to absorb a gas is in direct proportion to the quantity
employed.

Such are the principal circumstances that occurred to me as
requiring to be investigated, and to be at the same time capable
of affording results that may admit of general application where-
ever coal gas is employed as a source of light.   There are others
of more limited utility that may be left to be determined by those
persons who are interested respecting them; such as the prefer-
ence due to different varieties of coal as sources of gas, and
sometimes even to other inflammable substances, which, on
account of local situation, may be entitled to preference over
coal.   The facts which have been stated supply also data for
deciding other questions, which may be suggested by circum-
stances of partial interest; for example, whether it may not be
adviseable, in some cases, to collect only the first portions of
gas; or, if all be collected, to reserve different portions apart
from each other, and to apply them to appropriate uses.   Thus,
when coal gas is conveyed in portable gasometers to a distance
(as is now practised by Mr. Lee in supplying his house two miles
from the manufactory[†]), it will be important to select that gas,
which in a given volume has the highest illuminating power, and
which, therefore, requires vessels of the smallest capacity for its

* Nicholson's Journal, 8vo. vii. 297, and Thomson's *Annals*, vii. 214.

† A small carriage upon springs conveys two square close gasometers made of
wrought iron plates, and each containing 50 cubic feet of perfectly purified gas,
equivalent together to about 6 lbs. of tallow.   Each gasometer weighs about
160 lbs. and has a valve at the bottom, which is opened by the upright main pipe,
the moment the gasometer is immersed in the pit.   The strength of one man is found
to be sufficient for the labour of removing the gasometer from the carriage to its
place.

conveyance. Having, I hope, furnished documents for solving questions of this sort, I shall proceed to describe in what manner the facts were ascertained.

Method of Analysis.

1. *Determination of the Proportions of Carbonic Acid and Sulphuretted Hydrogen Gases in Coal Gas.*—In experiments formerly made on this subject, I employed the agency of chlorine to condense both these impurities, and estimated how much of the absorption was due to each, by a rule which I have stated.* Recent experience, however, has led me to distrust this method; and after comparing the effects of several other agents, by experiments on mixtures of known composition, I now prefer the white carbonate of lead, precipitated from acetate of lead by carbonate of ammonia without heat, and, therefore, fully saturated with carbonic acid. This precipitate it is better not to dry, but, after washing it sufficiently, to leave it under as much water as will give it, when wanted for use, a due degree of fluidity. This mixture may be applied by means of a tube of the capacity of a cubic inch, divided into 100 equal parts, and accurately ground into a short and wider piece of tube, which ought not to contain more than three or four-tenths of that quantity. The wider tube being filled with the fluid carbonate of lead, and placed with its mouth upwards under water, the graduated measure full of gas is fitted to it ; and the gas and liquid are brought into contact by alternately inverting the two tubes, all violent agitation being carefully avoided. The sulphuretted hydrogen is thus absorbed, and the carbonic acid, being left untouched, is afterwards taken out from the same portion of gas by a similar use of solution of pure potash.

2. *To ascertain the Proportion of Olefiant Gas in the Residue left by Potash.*—From 25 to 30 hundredths of a cubic inch of chlorine gas are passed into a tube of the diameter of about $\frac{3}{10}$ths of an inch, accurately divided into hundredths of a cubic inch ; and the volume of the chlorine is noted when actually in the tube, to avoid errors from its absorption in rising through the water. To this is admitted half a cubic inch (equivalent to 50 measures) of the gas under examination, and the mixture is left, excluded from the direct light of the sun, and perfectly quiescent, for 15 minutes. At the expiration of this time, the remainder is noted, and the diminution which has taken place being divided by 2, the quotient shows the quantity of olefiant gas in 50 measures of the mixture. This process, I am aware, however, does not give results of perfect accuracy ; for, in addition to other sources of fallacy, I find that chlorine begins to act on carburetted hydrogen much sooner than is generally supposed,† though

* Phil. Trans. 1808, p. 295.
† While this sheet was passing through the press, I have noticed a passage in

within the period mentioned, and in such narrow tubes, it does. not occasion a sensible diminution of bulk. The method described may, therefore, be considered as affording a tolerably near approximation to the proportion of olefiant gas ; and as all the varieties of coal gas were subjected to the test under precisely the same circumstances, the errors must have been of nearly the same amount in all cases, and cannot materially interfere with the fair comparison of the different specimens of coal gas, so far as respects their proportion of olefiant gas.

3. *To ascertain the Quantity of Combustible Matter* in gas which had been deprived only of sulphuretted hydrogen and carbonic acid, a mixture of the gas with a due proportion of oxygen gas was fired by the electric spark over mercury. This method I preferred to slow combustion, carried on with the apparatus which I have described in the Philosophical Transactions for 1808, solely because, when a great number of experiments are necessary, as in this inquiry, the method of detonation is attended with a great saving of time. But on all occasions where only few experiments are required on gases of great combustibility, I prefer slow combustion, both on account of greater safety to the apparatus, and, from the quantities that may be consumed, of greater accuracy also. When rapid combustion is practised, I believe that, on the whole, more accurate results are gained by firing the gas at one operation properly conducted, than at two. The latter method seems to have been preferred by M. Berthollet ; but so far as my experience goes, it is more apt to precipitate charcoal from the gas.

To burn each measure of the early and more combustible products of gas, I employed from three to four measures or upwards of oxygen gas, the degree of purity of which had been ascertained. The volume being noted after firing, and again after agitating the residue with liquid potash, the last diminution showed the quantity of carbonic acid. The gas left by potash was next analyzed by combustion with a due proportion of pure hydrogen,* which showed how much of the residue was oxygen, and how much azotic gas. If more azote was found than had been introduced as an impurity of the oxygen gas, it was considered as having formed a part of the combustible gas. A single experiment on any kind of gas was never relied upon ; and to ensure accurate results, the same gas was fired with different proportions of oxygen. Deducting the pure oxygen found in the residue, from its quantity at the outset, the volume of oxygen gas was learned, which had been spent in saturating a given measure of combustible gas.

Mr. Brande's Manual of Chemistry (p. 156 n.), from which it appears that the speedy action of chlorine on carburetted hydrogen had been observed by Mr. Faraday.
    * The method of doing this is given in my Elements of Chemistry, vol. i. chap. v. sect. vi.

In gases free from all admixture with carbonic oxide, it is easy to know how much of the oxygen consumed has been spent in saturating the charcoal; for as oxygen gas by conversion into carbonic acid suffers no change of volume, the quantity which has combined with the charcoal is exactly represented by the volume of carbonic acid produced by the combustion. For example, as 100 measures of olefiant gas afford by detonation 200 of carbonic acid, 200 measures of oxygen must have united with the charcoal of the olefiant gas. But beside these 200 measures, an additional 100 measures of oxygen are found to be consumed, and these must have combined with hydrogen, the other ingredient of the gas, the volume of which in its full state of expansion would be 200 measures, as determined by the fact, that oxygen gas uniformly takes for saturation double its volume of hydrogen gas, and no other proportion.

Nature of the Gas from Coal.

The opinion which I formerly advanced on this subject,[+] though opposed by writers of so much authority as M. Berthollet and Dr. Murray, still appears to me to be much more probable, than that the varieties of gas from inflammable substances, which may be almost infinitely diversified by modifications of temperature, are, as those philosophers suppose, so many distinct compounds of hydrogen and charcoal, or of hydrogen and charcoal in combination with oxygen. The reasons that induce me to abide by my original view of the subject are the following:

1. We are acquainted with two distinct and well characterized compounds of hydrogen and charcoal, in one of which a given weight of charcoal is united with a certain quantity of hydrogen, and in the other with double that quantity. Besides these two, no other compound of those two elements has been hitherto proved to exist.

2. It is inconsistent with experience that two bodies which, like hydrogen and charcoal, unite by an energetic affinity, should combine in all possible proportions. On the contrary, it is to be expected from analogy in general, and from that of the compounds of charcoal and oxygen in particular, that hydrogen and charcoal unite in few proportions only, and in such a manner that these proportions are multiples or divisors of each other by some entire number.

3. All the phenomena may be satisfactorily explained by supposing the gas from coal, and from other inflammable substances, to be mixtures of this kind. For example, referring to the one hour's gas in the first table, we shall find that it contains, in 100 measures, 18 of olefiant gas, which require for combustion 54 measures of oxygen, and afford 36 of carbonic acid. The same gas contains also $77\frac{1}{4}$ measures of another inflammable gas,

in the combustion of which 210 — 54 = 156 measures of oxygen have been spent, and which have afforded 112 — 36 = 76 measures of carbonic acid. This is as near an approach as can be expected to the properties of carburetted hydrogen, the 77$\frac{1}{2}$ measures having consumed very nearly twice their bulk of oxygen, and given an equal volume of carbonic acid. We may, therefore, consider the early products of the gas from cannel as a mixture of about one volume of olefiant gas and four volumes of carburetted hydrogen.*

The early product of gas from Clifton coal does not admit of being thus theoretically resolved into a mixture of olefiant and carburetted hydrogen gases only. For after deducting from the oxygen consumed (164 measures) that spent in saturating the olefiant gas (10 × 3 = 30) we have only 134 measures of oxygen left for the combustion of 90 measures of inflammable gas. These 90 measures, it appears, afford 91 — 20 = 71 measures of carbonic acid. This portion of the gas does not, therefore, answer to the characters of carburetted hydrogen, since it neither gives an equal volume of carbonic acid, nor consumes a double volume of oxygen. In this case and a variety of similar ones, we can only at present explain the phenomena, by comparing them with hypothetical mixtures of the different known gases. As an example, I shall describe the particulars of the combustion of the first product of Clifton coal, and endeavour to explain the results in the manner which has been suggested.

Measures of the gas 11
Mixed with oxygen .... 39 = 37 pure oxygen + 2 azote.

Total. 50
Volume after firing. 31
Ditto after washing by potash 21 = 19 oxygen + 2 azote.

18 oxygen consumed.

In this case, the diminution by firing is 19 measures ; that by potash, which denotes the carbonic acid, 10 measures ; and the gases consumed are 11 + 18 = 29. Let us examine what mixture of gases will account for the appearances.

M. of infl. gas.	Take oxygen.	Give carb. acid.	Dimin. by firing.
1·1 olefiant.....	3·3	2·2	2·2
7·0 carb. hydr ..	14·0	7·0	14·0
1·0 carb. oxide..	0·5	0·5	1·0
2·0 hydrogen. ..	1·0	—	3·0
11·1	18·8	9·7	20·2

* I am perfectly aware of the importance of taking the specific gravity of mixed gases, as one datum for determining their proportion in any mixture; but I was prevented from ascertaining it in these experiments by the state of the neces-

The sums of the numbers thus theoretically obtained do not, it is true, exactly correspond with the experimental ones ; but they approach as nearly as, from the nature of the subject, can be expected, the greatest disagreement (that in the diminution by firing) not much exceeding $\frac{1}{40}$th of the observed amount.

In a similar manner we may explain the composition of the lighter and less combustible products obtained at advanced periods of the distillation. For example, a portion of the last product of gas from cannel, distilled in a glass retort, gave the following results :

Measures of gas 20
Mixed with oxygen . . . . 30 = 28 pure oxygen + 2 azote
                                    ——
            Total. 50
Fired 22
Washed with potash . . . . 18 = 14·7 oxygen + 3·3 azote
                                    ———
                                    13·3 oxygen spent.

In this experiment, 1·3 more azote were found in the residuum than can be traced to the oxygen employed. The combustible gas was, therefore, only $18\frac{2}{3}$ measures ; the carbonic acid produced 4 ; the oxygen spent 13·3 ; and the diminution by firing, 28. The following supposed mixture will explain these facts :

Measures of	Take oxygen.	Give carb. acid.	Dim. by firing.
2 carb. hydr.	4	 2	 4
2 carb. oxide	2	 2	 2
15 hydrogen	$7\frac{1}{2}$	. . . . . . —	. . . . . . $22\frac{1}{2}$
——	——	—	——
19	$13\frac{1}{2}$	4	$28\frac{1}{2}$

In this instance, the hypothetical constitution coincides even more nearly with the facts than in the former case. It must, indeed, be acknowledged that the explanation rests on hypothesis only ; but it is on an hypothesis which is perfectly consistent with a copious and increasing induction of facts, all tending to establish a limitation to the proportions in which bodies combine ; while the opposite explanation is at variance with this general law of chemical union.

sary apparatus, which was found, from long disuse, to have become unfit for the purpose. So far as respects the practical objects of this paper, the omission is of no consequence.

Article VII.

Analyses of Books.

Philosophical Transactions of the Royal Society of London,
for 1819, *Part I.*

This part contains the following papers :

I. *The Croonian Lecture.—On the Conversion of Pus into*
Granulations, or new Flesh. By Sir Everard Home, Bart.
V.P.R.S.

In the last volume of the Transactions, Sir Everard Home
endeavoured to explain how coagulated blood became vascular.
Carbonic acid gas, he informed us, is extricated at the moment
of coagulation. This gas gradually lengthens into a tube, which
is immediately covered by a coat, and thus converted into a
blood-vessel. He is of opinion that the same process goes on
during the conversion of pus into granulations, or new flesh.
Pus, he tells us, is analogous to the serum of blood. At first, it
contains no globules, but they gradually make their appearance
in it whether it remains on the surface of the sore, or be removed
upon some other surface. Mr. Bauer has observed that the
same formation of globules takes place in the serum of blood;
thus showing the analogy between serum and pus. The paper
is taken up in describing the appearances which are perceived
upon the surface of a healing sore, when left for about a quarter
of an hour exposed to the atmosphere. The coating of pus
coagulates, globules of carbonic acid gas make their appearance
in it. These are speedily converted into numerous anastomosing
vessels filled with red blood.

II. *On the Laws which regulate the Absorption of Polarized*
Light by Doubly Refracting Crystals. By David Brewster,
LL.D. F.R.S. Lond. and Edin.

If to one side of a rhomboid of colourless calcareous spar we
fasten a circular aperture of such magnitude that the two images
of it appear distinctly separated when viewed through the spar,
we shall find that when the spar is exposed to common light,
the two images are equally colourless, and of the same intensity
in all positions; so that the *ordinary image* contains half the
quantity of transmitted light, and the extraordinary image like-
wise half the quantity of transmitted light. When the rhomboid
is exposed to polarized light, the intensities of the two pencils
are together equal in every position to the whole transmitted
light. Hence the rays which leave one of the images by a change
of azimuth pass over into the other image.

When the same experiment is tried with certain specimens of
yellow calcareous spar, the results are different. The two images
differ both in colour and intensity; the extraordinary image

having an orange-yellow hue, while the colour of the ordinary image is yellowish-white. This difference of colour is related to the axis of the crystal, and increases with the inclination of the refracted ray to the short diagonal of the rhomb. It is a maximum in the equator, while along the axis the two images have exactly the same colour and intensity. The author shows that there is an interchange of rays. The extraordinary force carries off several of the yellow rays from the ordinary image ; while, at the same time, the ordinary force takes to itself some of the white rays from the extraordinary image.

When the rhomboid is exposed to polarized light in the position in which the ordinary image vanishes, the extraordinary image is *orange-yellow*, and in the position in which the extraordinary image vanishes, the ordinary image is a *yellowish-white*. It follows from this, that a portion of the ordinary pencil was absorbed in the first position, and a portion of the extraordinary pencil in the second position.

The author examined coloured crystals of zircon, sapphyr, ruby, emerald, beryl, rock crystal, amethyst, tourmaline, rubellite, idocrase, mellite, phosphate of lime, phosphate of lead, and observed similar appearances. Now these are a great proportion of all the coloured crystals with one axis of double refraction at present known to exist.

The general phenomena of absorption in crystals with two axes are nearly the same as those with one ; but the quantity of light which the ordinary and extraordinary forces interchange is regulated by new laws depending on the situation of the incident ray with respect to the two axes of double refraction. The author explains these laws, and gives a table of the different colours resulting from these absorptions in a variety of coloured crystals with two axes.

The author concludes from his observations that the colouring particles of crystals, instead of being indiscriminately dispersed throughout their mass, have an arrangement related to the ordinary and extraordinary forces which they exert upon light. In some specimens, the extraordinary medium is tinged with the same colouring particles, and with the same number of them as the ordinary medium ; but in other specimens of the same mineral, the extraordinary medium is either tinged with a different number of particles of the same colour, or with a colouring matter entirely different from that of the ordinary medium. In certain specimens of topaz, the colouring matter of the one medium is more easily discharged than that of the other. Hence the reason why such topazes become pink when exposed to a red heat.

III. *Observations on the Decomposition of Starch at the Temperature of the Atmosphere by the Action of Air and Water.* By Theodore de Saussure, Professor of Mineralogy in the Academy of Geneva, Correspondent of the Royal Institute of France, &c.

The author of this memoir is of opinion that an examination of the action of vegetable substances on each other and of the effects produced upon them by the action of air and water, is the best method of investigating various effects of vegetation ; or at least if it does not answer that purpose, it will lead to important experiments respecting the theory of fermentation. Starch had scarcely been examined under this point of view, or only indirectly, and in a way quite insufficient to enable us to deduce the requisite consequences. It had been observed that the seeds of corn formed sugar during germination; and that this does not happen unless they be impregnated with water, and air have access to them. Hence it was concluded that the oxygen gas which disappeared, producing carbonic acid gas, was the principal agent in the conversion of the starch into sugar. Vogel had exposed a mixture of starch and sugar to the action of a boiling heat for four days. The mixture became very fluid. It was filtered. The filtered liquid being evaporated left a bitter mucilage, which had not the least of a saccharine taste. A horny looking matter remained on the filter. Kirchoff has discovered that if one part of dry pulverized gluten be mixed with two parts of starch made into a paste with water, and the mixture be digested for 10 or 12 hours at the temperature from 122° to 167°, the starch is partly converted into sugar. Hence he has concluded that the conversion of starch into sugar takes place during germination.

Such was the state of our knowledge before the experiments which Saussure relates in the present paper. He mixed together 20 grammes of the best wheat starch, and 12 times the weight of water, so as to form a thin paste. This was put into a large flat cylinder forming a layer to the depth of two centimetres (0·8 inch nearly). It was covered by a large receiver, below which the atmospherical air had easy access, and left at rest for two years in a place in which the temperature rose as high occasionally as $72\frac{1}{4}°$. At the end of this period, it was a grey-coloured liquid paste, covered with mucors, and almost without smell. It produced no change on vegetable blues, and could no longer be employed to paste substances together. After being dried in the temperature of the atmosphere, its weight was considerably diminished. If we suppose its original weight to have been 100, it was reduced to 76·2 dried at the temperature of the atmosphere, or to 80·46, supposing both dried at the temperature of boiling water. This residual matter was carefully analyzed, and found to consist of the following substances :

Sugar,
Gum,
Amidin,
Starchy lignin,
Lignin mixed with charcoal,
Starch undecomposed.

The *sugar* possessed the characters of the sugar made from starch by means of sulphuric acid.

The *gum* possessed the following properties : It was transparent, and almost colourless, when formed without the contact of air ; but when the starch became covered with mucors, the gum was yellow, and rather too soft to be reduced to powder. One hundred parts of this gum at 66°, when exposed to the heat of 212°, lost 11·75 of their weight. It does not absorb moisture from the air, nor is it altered by exposure to the atmosphere; but its aqueous solution becomes gradually putrid, depositing a thick mucous matter. It is insoluble in alcohol, but soluble in water in every proportion. Two parts of water and one of the gum form a very fluid solution, but it becomes viscid and thready when the weight of the gum exceeds that of the water. A solution of one part of the gum in ten parts of water is neither precipitated by acetate of lead, nor subacetate of lead, nor the decoction of nutgalls, nor silicate of potash. It does not alter the colour of the infusion of litmus. It does not alter the colour of aqueous solution of iodine. It is slightly precipitated by barytes water. It does not form mucic acid when treated with nitric acid. It possesses most of the characters of the gum into which starch is converted by roasting.

Saussure has applied the term *amidin* to a substance which he considers as intermediate between gum and starch. The word is formed from the French term *amidon* (starch). As it is contrary to rule to permit the nomenclature of chemistry to be obscured by words borrowed from living languages, it is obvious that if a new term be requisite for this substance, we must call it *amylin* (from the Latin word *amylum*), used by the moderns for *starch*. It is obtained from the residue left by the spontaneous decomposition of starch after it has been treated with a sufficient quantity of cold water to dissolve every thing soluble in that liquid. Boiling water dissolves the amylin, and it may be obtained by evaporating the solution to dryness. It is obtained either in irregular, opaque fragments, or of a yellow-pale semitransparent brittle substance, according to the mode of conducting the evaporation. It is insoluble in alcohol. Cold water dissolves about one-tenth of its weight of it, and forms a colourless and very fluid liquid. Water of the temperature of 144° dissolves it in any proportion, and retains in solution, after cooling, a much greater proportion than can be dissolved in cold water. The decoction may be concentrated till it contains one-fourth of its weight of amylin in solution without becoming muddy or gelatinizing on cooling, which is not the case with starch. When the liquid is more concentrated, the amylin precipitates in part on cooling in the state of a white opaque matter; but it is redissolved on heating the water to 144°. In this respect it approaches inulin. The solution of amylin containing one-tenth of its weight of this substance, assumes a blue colour,

when mixed with the aqueous solution of iodine, and presents with this re-agent all the effects of starch. It is coagulated into a white paste by the subacetate of lead; but not by the neutral acetate. It is copiously precipitated by barytes water, but not by lime water, nor decoction of nutgalls. It dissolves in the aqueous solution of potash. This liquid is very fluid, and wants the viscidity of the solution of starch in potash. The weak acids precipitate the amylin from it with all its properties. Alcohol likewise throws down a copious precipitate, which, however, retains a portion of the alkali, and does not strike a blue with iodine till an acid is added to it.

The starchy lignin was obtained from the residue of the spontaneous decomposition of starch, after that residue had been deprived of every thing soluble in cold water, hot water, alcohol, and very dilute sulphuric acid, by digesting it in 10 times its weight of an alkaline ley containing one-twelfth of its weight of potash. A brown liquid solution is obtained, from which dilute sulphuric acid precipitates the starchy lignin under the form of a brown, light combustible substance, having the lustre of jet. It gives a blue colour to the aqueous solution of iodine. This last property, together with its solubility in a weaker alkaline ley, distinguishes starchy lignin from common lignin.

During the spontaneous decomposition of starch, the bulk of the air undergoes no change; but a small portion of its oxygen is slowly converted into carbonic acid gas. Fifteen grammes ($231\frac{2}{3}$ gr. troy) of starch mixed with 12 times its weight of water produced in two months, in the temperature of $72\frac{1}{4}°$, only 50 cubic centimetres (19·68 cubic inches) of carbonic acid gas. Thus the action of the oxygen of the air is confined to the abstraction of carbon. The loss of weight which the starch sustains is much greater than can be accounted for by the carbon abstracted by the oxygen of the air. Hence the starch must lose a considerable portion of its weight by giving out oxygen and hydrogen under the form of water.

To ascertain the difference in the result when the starch was allowed to decompose in the air and in a vacuum, the experiment was repeated both ways for 38 days in a temperature of about $72\frac{1}{4}°$. The following table exhibits the results of these two experiments:

One hundred parts of starch decomposed without the contact of air, yielded

Sugar	47·4
Gum.	23·0
Alylin	8·9
Starchy lignin.	10·3
Lignin mixed with charcoal	Trace
Starch not decomposed.	4·0
	93·6

One hundred parts of starch decomposed in contact of air yielded

Sugar	49·7
Gum.	9·7
Amylin.	5·2
Starchy lignin.	9·2
Lignin mixed with charcoal	0·3
Starch not decomposed.	3·8
	77·9

These experiments being repeated with potatoe starch, and continued only for 42 hours, yielded the following products :

	In vacuo.	
Starch	35·4	30·4
Gum.	17·5	17·2
Amylin.	18·7	17·0
Starchy lignin..................	7·0	4·4
Lignin with carbon.	Trace	0·2
Starch undecomposed.	9·4	9·3
	88·0	78·5
Loss during analysis.	6·0	
	94·0	

When the starch was decomposed in vacuo, it rather gained than lost weight. The apparent loss was owing to the starch not having been dried before the experiment at a temperature so high as 212°. No water was formed, and the carbonic acid evolved was mixed with a portion of hydrogen gas, pure, or at least containing only a very small portion of carbon.

IV. *On Corpora Lutea.* By Sir Everard Home, Bart.

According to the author of this paper, the corpora lutea make their appearance at the age of puberty. They are composed of convolutions similar to the brain ; their use is to form the ova. These, when formed, are expelled by the corpora lutea, whether impregnation has taken place or not. Sometimes the ovum remains in the corpus luteum without being expelled. In such cases (supposing impregnation) a fœtus will be formed in the ovarium.

V. *Remarks on the Probabilities of Error in physical Observations, and on the Density of the Earth, considered especially with Regard to the Reduction of Experiments on the Pendulum.* By Thomas Young, M.D. For. Sec. R.S.

This paper consists of four parts. The first part, which is by far the longest and most elaborate, is employed in the application of the science of probabilities to estimate the advantage of multiplied observations. After some judicious observations on

the application of the doctrine of probabilities to moral and political observations, he enters upon its application to experimental observations, and shows that when we have ascertained the mean error, that will amount nearly to our deviation from accuracy. But he observes, that in experiments so many things are of necessity left out of view that the application of the doctrine of probabilities to them is not likely to lead to any advantageous result.

The second section is on the mean density of the earth. He shows, as had been already done by Laplace, that the pressure of the strata composing the earth is fully sufficient to account for the greater density of the central parts above the superficial, without supposing the materials in the centre to be different from those at the surface.

The third section is an investigation of the effects of the irregularities at the earth's surface upon the plumb line, and on gravitation. In the fourth section, he shows that Laplace's theorem for the length of the convertible pendulum rolling on equal cylinders, may be deduced from an elegant investigation of Euler in the Nova Acta Petropolitana for 1788, p. 145.

VI. *On the Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.* By William Scoresby, Jun. Esq.

Mr. Scoresby gives a table of magnetical observations which he made on board the Esk, in his voyage to the Greenland whale fishery in 1817. From these observations, and from the lucid remarks of Capt. Flinders, he deduces a number of inferences, some of the most important of which are the following:

1. Such parts of the iron employed in the construction of ships as have a perpendicular position are magnets, the upper ends being south, the lower north poles in our hemisphere, and the contrary in the southern hemisphere.

2. The combined influence of all this iron seems to be concentrated into a kind of magnetic focus of attraction, the principal south pole of which he conceives to be situated near the middle upper deck, but nearer the stem than the stern.

3. This focus of attraction so influences the compass needle that it is subject to an anomaly or variation from the true meridian, different from what is observed by a compass on shore; the north point of the compass being constantly drawn towards the focus in our hemisphere, and the south point in the opposite hemisphere.

Mr. Scoresby, sen. observed 20 years ago, that a ship beating to the northward with a north wind appeared to lie nearer the wind than when beating to the southward with a southerly wind. This he ascribed to the attraction of the ship upon the compass. On this account, the author has been in the habit of allowing only 2 or 2¼ points' variation on the passage outward to Greenland with a northerly or north-easterly course, but generally three

points of variation on the homeward passage, when the course steered was S.W. or S.W. by W. Without this difference of allowance, a Greenland ship outward bound will be generally found to be to the eastward of her reckoning, and homeward bound will be even four or five degrees to the eastward of it.

4. This anomaly in the variation of the compass is liable to change with every alteration in the dip of the needle, in the position of the compass, or in the direction of the ship's head.

It would appear from Mr. Scoresby's trials, that the magnetic intensity is greater in England than in Greenland; at least the vibrations of the dipping needle were quicker in England than in Greenland. An oscillation performed in England in five seconds took up in Greenland s x seconds.

5. The anomaly of variation bears a certain proportion to the dip of the needle, being greater where the dip is greatest, decreasing as the dip decreases, and disappearing altogether on the magnetic equator.

6. A compass placed near the stern amid ships of the quarter deck is subject to the greatest anomaly when the ship's course is about west or east, because the focus of attraction then operates at right angles to the position of the compass needle; but the anomaly disappears when the course is about north or south, because the focus of attraction is then in a line with, or parallel to, the compass needle, and consequently has no power to deflect it from its direct position.

7. The greatest anomaly with the compass in the position last described being ascertained by observation, the error on every other point of the compass may be easily calculated; the anomalies produced by the attraction of the iron in the ship being found to be proportionate to the sines of the angles between the ship's head and the magnetic meridian.

8. A compass placed on either side of the ship's deck directly opposite to, or abreast of, the focus of attraction, gives a correct indication on an east or west course; but is subject to the greatest anomaly when the ship's head is north or south.

9. A compass placed within six or eight feet of a capstern spindle, or other large mass of wrought iron, foregoes, in a great measure, the influence of the focus of attraction, and submits to that of the nearer body of iron.

10. When the iron in a ship is pretty equally distributed throughout both sides, so that the focus of attraction occurs in midships, a compass placed in the midship line of the deck (drawn longitudinally) will be free from any anomaly from one end of the ship to the other, when the course is north or south; but on every other course, anomaly will generally appear increasing as the angle between the ship's head and the magnetic meridian increases until the error is at a maximum, when the course is east or west.

11. As a compass placed on the midship line of the deck is subject to no anomaly fore and aft, in certain ships on a north and south course ; and as a compass on either side of the ship opposite to the focus of attraction shows no anomaly on a west or east course, the intersection of the line joining the two situations in opposite sides of the ship with the midship line traced fore and aft, will probably point out a situation directly over the top of the focus of attraction, when no anomaly in any course whatever will appear.

12. The anomaly of variation is probably greater in men of war and in ships which contain large quantities of iron ; but it exists in a very considerable degree also in merchantmen, where iron forms no part of the cargo, especially in high latitudes where the dip of the needle is great.

VII. *On the Genus Ocythöe; being an Extract of a Letter from Thomas Say, Esq. of Philudelphia, to William Elford Leach, M.D. F.R.S.*

This paper gives a description of a fine species of ocythöe found in the stomach of a dolphin, which the author considers as new, and distinguishes by the name of ocythöe punctata. He is of opinion that the ocythöe is a parasitical animal, because the shell in which it is found is not closely adapted to its body, as is the case with the shells of all known shell fish.

VIII. *On Irregularities observed in the Direction of the Compass Needles of his Majesty's Ships Isabella and Alexander, in their late Voyage of Discovery, and caused by the Attraction of the Iron contained in the Ships.* By Capt. Edward Sabine, of the Royal Regiment of Artillery, F.R.S. &c.

Mr. Wales, astronomer to Capt. Cook in his second voyage, was the first person whose observations led to the inference that the anomalies observed on ship-board in the position of the compass, were not owing to imperfections in the azimuth compass, as had been supposed, but to the effect of the iron in the ship upon the needle. This subject was afterwards investigated with much acuteness and industry by Capt. Flinders during his survey of the coast of New Holland. He succeeded in being able to apply a correction in his own ship. After his return, he was permitted by the Lords of the Admiralty to prosecute the investigation still further by trying the irregularities in different ships in various harbours. Capt. Flinders published a short paper on the subject in the Philosophical Transactions, and a more detailed statement in the second appendix to his Voyage to Terra Australis. There are three points in his statement chiefly worthy of attention from their practical importance. Capt. Sabine's object in this paper is to point out how far Capt. Flinders's observations have been confirmed by those made in the Isabella and Alexander.

First, Capt. Flinders found that in every ship a compass

would differ very materially from itself on being removed from one part of a ship to another. Hence he was led to confine his compass to one particular spot. He selected the binnacle for conveniency. It was exactly midships. This observation of Capt. Flinders was fully corroborated in the Isabella and Alexander.

Secondly, Capt. Flinders found that in his compass, permanently fixed as described, no error took place when the ship's head was on the magnetic north or south points ; showing that at such times the attraction of the ship and of magnetism was in the same line of direction. The maximum of error also took place when the ship's head was at right angles to these points ; namely, at east or west ; being, however, in opposite directions, in excess of the true variation on the one side, and in defect on the other ; so that the extreme difference occasioned by altering the course from east to west, or the reverse, would be twice the error at either. On the intermediate points, the ratio of the error to its maximum was as the sine of the angle between the ship's head and the magnetic meridian to the sine of eight points, or radius, or sufficiently near to admit of corrections being calculated for every course, when the errors on a single one were known by observation.

Though Capt. Flinders was induced from his observations to conclude that the point of no error was when the ship's head coincided with the magnetic meridian, yet this did not hold either in the Isabella or the Alexander. Indeed in the Alexander it was nearly at right angles to that meridian. The point of no error did not coincide in the two ships. The reason was, that in the Isabella the compass of observation was raised a considerable height above the deck, while in the Alexander it was upon the deck. Capt. Sabine, in consequence of this, proposes to alter Capt. Flinders's rule to the following :

" The error produced in any direction of the ship's head will be to the error at the point of the greatest irregularity, as the sine of the angle between the ship's head and the points of no error to the sine of eight points or radius."

Thirdly, Capt. Flinders's experience in the Investigator showed that the maximum of error in the same compass would be different in different parts of the world, although the use of the compass was confined to one particular spot in the ship, and every precaution taken to avoid an interference with the distribution of the ship's iron.

Capt. Flinders observed that the error increased with the dip, and he conceived that the amount of the error under any one dip being known, the amount may be calculated for any other dip by using as a multiplier the decimal expression of the proportion which the error in the one ascertained instance may have borne to the dip. But it is obvious from the observations made in the

Isabella and Alexander, that the error increases in a much greater ratio than this. Capt. Flinders conceived that this increase of the error with the dip was owing to the increased magnetic energy of the iron in the ship as we approach the magnetic pole; but Capt. Sabine suggests that the cause of the increase of error is the diminution of the directive power in consequence of the dip. Now it is pretty evident that if this cause is adequate to produce the effect, the rate of error might be subjected to mathematical calculation.

IX. *Some Observations on the Formation of Mists in particular Situations.* By Sir H. Davy, Bart. F.R.S. V.P.R.I.

When water above the temperature of 40° is cooled, it sinks below the surface, and its place is supplied by hotter water from below. Hence rivers in clear nights cannot be cooled down so much by radiation as the land on each side of them. The consequence will be that the temperature of the river during the night will be several degrees higher than that of the contiguous land. The air above each will participate in this inequality of temperature. Now whenever the atmosphere above the banks of rivers is cooled down several degrees below the air over the river, and the two are so situated as to mix a little together, a mist will be formed over the bed of the river. Such is the explanation of the formation of mists in autumn above the beds of rivers given by Sir H. Davy.

X. *Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force, made during the late Voyage in Search of a North-west Passage.* By Capt. Edward Sabine.

The intensity of the magnetic force increased with the latitude; but was not proportional to the increase of latitude. The following table, indicating the time taken up by a dipping needle in making 100 vibrations in different latitudes and longitudes, gives the result of Capt. Sabine's observations on this subject :

Latitude, N.	Longitude, W.	In the meridian.	First arc.	Perpendicular to the meridian.	First arc.	
51° 31'	0° 08'	0' 00''	0°	8' 18·3''	90	Regent's Park, London.
60 09	1 12	7 49¼	74	7 59·5	90	Shetland.
68 22	53 50	7 20	83	7 33	90	On ice, Davis's Straits.
70 26	54 52	7 21	83	7 26	90	Hare Island.
75 05	60 23	7 27½	84	7 26	90	On ice, Baffin's Bay.
75 51½	63 06	7 23½	84	0 00	—	On ice, Baffin's Bay.
76 45	76 00	7 15	85	7 26	90	On ice, Baffin's Bay.
76 08	78 21	7 16	85	7 18	90	On ice, Baffin's Bay.
70 35	66 55	7 16	83	7 18·5	90	On ice, Davis's Straits.
51 31	0 08	8 02	70	8 18	90	Regent's Park, London.

The dip of the needle likewise increased with the latitude. The following table exhibits the observation:

1818.	Lati-tude, N.	Longi-tude, W.	No. of observ.	Observer.	Dip.	
April 18	51° 31'	0° 08'	16	Capt. Kater.	70° 34' 39"	Regent's Park, London.
30	60 09½	1 12	14	Capt. Sabine.	74 22 48	} Brassa Island, Shetland.
May 1	60 09½	1 12	12	Lieut. Parry.	74 20 10	
June 9	68 22	53 50	12	Capt. Sabine.	83 08 07	On ice.
19	70 26	54 52	14	Ditto.	82 48 47	Hare Island.
July 8	74 04	57 52	10	Ditto.	84 09 15	Baffin's three islands.
23	75 05	60 03	10	Lieut. Parry	84 24 57	} On ice.
23	75 05	60 03	10	Capt. Sabine.	84 25 15	
Aug. 2	75 51½	63 06	10	Ditto.	84 44 30	Ditto.
4	75 50	64 47	10	Ditto.	84 52 06	Ditto.
19	76 32	73 45	10	Ditto.	85 44 23	Ditto.
20	76 45	76 00	14	Lieut. Parry.	86 08 53	Ditto.
20	76 45	76 00	14	Capt. Sabine.	86 09 33	Ditto.
25	76 08	78 29	16	Ditto.	85 · 59 31	Ditto.
Sept. 11	70 35	66 55	10	Ditto.	84 39 21	Ditto.
Nov. 3	60 09½	1 12		Lieut. Parry.	74 21 06	} Brassa Island, Shetland.
3	60 09½	1 12	14	Capt. Sabine.	74 21 47·15	
1819.						
March	51 31	0 08	16	Ditto.	70 33 16	Regent's Park, London.

The following table exhibits the variation of the compass in different latitudes and longitudes according to the observations of Capt. Sabine, with an azimuth compass contrived by Capt. Kater. These observations are entitled to particular attention, because they enable us to point out, with considerable precision, the position of one of the magnetic poles. It must be very nearly N. latitude 75° 59', and longitude 64° 32' W. from Greenwich. If we suppose this latitude and longitude to be correctly given, the true position of this magnetic pole may be easily found:

1818.	Mean Greenwich time.	Latitude, N.	Longitude, W.	Observed altitude.	Observed azimuth.	True azimuth.	
June 9	22ʰ 0′ 00″	68° 23¼′	53° 47′	48° 23′ 15″ ⊙‖	156° 27′	88° 55½′	67° 31½′ W.
11	23 20 00	68 14	54 15	63 20 ·58 ⊙‖	175 47	108 07	67 40
12	0 0 00	68 14	54 15	70 02 33 ⊙‖	186 16	118 12	68 04
17	8 20 00	70 26¼	54 52	56 15 36 ⊙‖	332 54	260 08	72 46
17	8 30 00	70 26¼	54 52	55 32 34 ⊙‖	334 05	261 11½	72 53¼
18	8 28 00	70 26¼	54 52	55 20 52 ⊙‖	332 52	261 33	71 19
18	8 35 00	70 26¼	54 52	{55 33 45 ‖⊙‖	334 24	263 10½	71 13¼
18	8 35 00	70 26¼	54 52	{53 57 35 ‖⊙‖	334 24	263 10½	71 39¼
18	23 20 00	70 26¼	54 13	64 43 18 ⊙‖	184 35	112 55¼	71 13¼
27	9 00 00	71 02½	54 13	50 18 13 ⊙‖	343 26¾	268 13⅔	75 20⅔
27	9 20 00	71 02½	54 13	49 18 20 ‖⊙‖	346 33⅓	271 13	75 31
27	9 40 00	71 02½	54 13	47 07 00 ‖⊙‖	349 54¼	274 23	75 35
27	10 00 00	71 02½	54 13	44 36 40 ‖⊙‖	353 23	278 00	75 48
27	10 20 00	71 02½	54 13	42 07 11 ‖⊙‖	357 24⅓	281 36⅓	78 55
July 4	10 45 00	72 44½	56 49	37 18 47 ‖⊙‖	7 13⅓	288 18⅔	80 01
6	9 12 00	73 22½	57 32	49 10 00 ‖⊙‖	348 16	268 15	80 43¼†
12	23 30 00	74 01⅓	57 52	55 11 52 ‖⊙‖	186 49¾	106 05⅔	84 33
21	10 05 00	74 58	59 16	38 49 57 ‖⊙‖	0 33¼	276 00⅓	86 59¼
22	23 02 00	75 04	60 03	45 46 00 ⊙‖	185 16	98 16⅓	88 18⅓
28	10 20 00	75 28	60 34⅛	35 05 10 ‖⊙‖	7 40	279 21⅓	87 37⅓
30	8 17 00	75 32	61 00	48 23 57 ⊙‖	336 24¼	248 46⅔	88 13¼
30	10 20 00	75 32	61 00	36 29 35 ⊙‖	0 55⅓	272 41⅓	88 13¼

* Observatory, Hare Island.　　　† On Baffin's three islands.

Table continued.

1818.	Mean Greenwich time.	Latitude, N.	Longitude, W.	Observed altitude.	Observed azimuth.	True azimuth.	W.
Aug. 2	10ʰ 00′ 00″	75° 44 1/4′	64° 00′	17° 33′ 15″ L	2° 26′	273° 25′	89° 01′
2	10 10 00	75 44 1/2	64 00	17 01 25 L	4 31	275 37 1/3	88 53 1/3
4	0 12 30	75 59	64 32	46 39 55 o‖	204 01	113 43 1/3	90 17 2/3
6	9 34 00	70 50 1/2	64 34	37 27 20 o‖	354 24	263 16 1/3	91 07 2/3
12	0 00 51	75 54 1/2	65 30	40 19 34 o‖	202 31 1/3	109 45 2/3	92 45 2/3
12	0 12 51	75 54 1/2	65 30	40 59 55 o‖	205 24 2/3	110 50 1/2	94 34 2/3
12	0 29 30	75 54 1/2	65 30	43 36 28 o‖	210 52 1/2	117 12 2/3	93 39 2/3
19	2 13 00	76 30	72 35	45 59 50 o‖	240 32	138 23	102 09
19	2 30 00	76 30	72 35	47 27 10 o‖	246 53 1/2	143 37 1/2	103 16
19	2 33 00	76 30	72 35	47 33 40 o‖	245 58 1/2	144 03 1/2	101 55
22	9 00 00	76 32 3/4	76 52 1/2	36 54 40 o‖	348 56	241 00	107 56
25	8 27 10	76 08 1/2	78 21	40 32 30 o‖	341 05 1/2	230 07	110 58 1/2
25	8 27 10	76 08 1/2	78 21	40 14 05 ‖o‖o	341 17 1/2	230 58	110 19 1/2
25	10 03 00	76 08 1/2	78 21	30 06 47 ‖o o‖	4 05	254 56	109 09
25	10 08 00	76 08 1/2	78 21	28 25 25 L	5 43 1/2	256 18	109 25 1/2
Sept. 11	8 30 00	70 35 1/2	66 55 1/2	13 27 30 L	330 44	244 06	86 38
11	8 36 00	70 35 1/2	66 55 1/2	12 59 47 L	332 58 2/3	245 36	87 22 2/3
11	8 40 00	70 35 1/3	66 55 1/2	12 39 33 L	333 19 1/2	246 41	86 38 1/3

In the column of " Observed altitude," o⩵ signifies the lower, and ⩵o the upper limb of the sun, the altitude being taken by reflection. L, the lower limb by the natural horizon.

XI. *On the Action of Crystallized Surfaces upon Light*. By Dr. Brewster.

Malus was of opinion that the forces which produce extraordinary refraction begin to act only after light has penetrated the surface of a doubly refracting body. The author had been induced, from experiments made before the perusal of Malus's book, to conclude that the polarizing forces extend beyond the surface of the crystal. This led him to repeat the experiments on which Malus's opinion was founded. The result of the investigation, which overturns the doctrine of Malus, is contained in this paper. If we take a six-sided prism of nitre, and observe a luminous object through two of its inclined surfaces that have a good polish, we perceive two distinct and perfectly formed images. If we now roughen these surfaces and cement upon each of them a plate of glass by means of balsam of capaivi, the character of the two images will be greatly changed. The image that has suffered the greatest refraction will be as distinct as before; but the other image will be either of a faint-reddish colour, or wholly invisible, according to the degree of roughness induced upon the refracting surfaces. If we substitute pure *alcohol* or the *white of an egg* for the balsam, the least refracted image will become distinct, and the most refracted image will be either a mass of nebulous light, or almost invisible. The reason of this is, that balsam of capaivi has nearly the same index of refraction as the ordinary image, but not of the extraordinary. It therefore removes the roughness of the surfaces as far as the ordinary image is concerned, but leaves the roughness for the extraordinary image. The index of refraction of alcohol and white of egg is nearly the same as that of the extraordinary image. It therefore removes the roughness as far as the extraordinary image is concerned; but leaves it with regard to the ordinary image. Similar experiments were made with calcareous spar and arragonite. The author draws from them the following conclusions :

1. The force of double refraction and polarization extends not only without the interior limit of the ordinary refractive force, but also without the surface of the crystal.

2. The force of double refraction and polarization emanates from the surface of bodies, though its intensity depends upon the inclination of the surface to the axis of the crystal.

3. The ordinary or the extraordinary image may be extinguished at pleasure in any doubly refracting crystal; and the crystal is thus converted into a singly refracting crystal, like certain specimens of agate.

4. In soft crystals that do not admit of a perfect polish, the distinctness of any of the two images may be made a maximum, by giving the crystal the best polish of which it is susceptible, and then cementing plates of glass upon its surface by a transparent cement of the same refractive power as that of the pencil,

which is to be rendered most distinct. If it is required to make the two images equally distinct, the refractive power of the cement must be a mean between that of the ordinary refraction and the extraordinary refraction, which corresponds to the angle which the refracted ray forms with the axis of double refraction.

5. All doubly refracting crystals consist of an ordinary and an extraordinary medium alternating with each other, and varying in density according to a law which the author has investigated, but not given in this paper. Dr. Brewster, in the subsequent part of this paper, shows that the change in the angle of polarization produced by the interior force, depends on the inclination of the reflecting surface to the axis of the crystal, and also on the azimuthal angle which the plane of reflection forms with the principal section.

This half volume terminates with a postscript to Dr. Young's paper, containing an investigation of the corrections for refraction. This investigation being entirely analytical, I must refer those readers who wish to know the important results obtained by this very acute philosopher to the paper itself, which occupies only four pages.

The usual meteorological journal for 1818 is given likewise in this half volume; the mean results of which are as follows: temperature, 53·5°; barometer, 29·88 inches, at the height of 81 feet above low water at Somerset House. No correction for temperature is introduced, which renders the barometrical heights given in the journal of little or no value.

Rain, 11·636 inches.

Mean variation of the needle in June, 24° 15′ 43″ W.

Dip about 70° 51′.

Article VIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Nov. 25.—Dr. Carson's paper, On the Elasticity of the Lungs, was concluded. After some introductory remarks, comprehending a popular description of the thorax and its contained viscera, the author proceeded to observe that the influence of the elasticity of the lungs on the circulation of the blood and on respiration, has been overlooked by physiologists. To ascertain the real force of the elastic power of these organs, Dr. C. connected with the tracheæ of several animals a glass syphon, so placed as to admit of pressure being exerted on the lungs by a column of water contained in it. An opening was then made into the cavity of the chest on both sides, and the height of the column of water in the tube was considered as equivalent to the pressure

exerted upon it by the elastic power of the lungs. From experiments conducted in this manner upon the lungs of the ox, Dr. C. considered it as clearly ascertained that in this animal the resilience of the lungs is more than equal to a column of water a foot and a half high. In a still more satisfactory experiment made upon the lungs of a dog, the column stood at 10 inches.

The paper was concluded with some remarks upon artificial respiration, and on the best means of ascertaining the actual quantity of air contained in the lungs.   •

Nov. 30.—On this day the annual meeting for the election of officers for the ensuing year took place, when the following noblemen and gentlemen were elected :

President.—Right Hon. Sir Joseph Banks, Bart. G. C. B. &c.

Secretaries.—W. T. Brande, Esq. and Taylor Combe, Esq.

Treasurer.—Davies Gilbert, Esq.

There remained of the old Council, Right Hon. Sir J. Banks, Bart.; W. T. Brande, Esq.; Taylor Combe, Esq.; Davies Gilbert, Esq.; Major-General Sir James Willoughby Gordon, K. C. B.; Sir Everard Home, Bart.; Sir Thomas Staunton, Bart.; William Hyde Wollaston, M.D.; and Thomas Young, M.D.

There were elected into the Council, William Blake, Esq. A.M.; John Earl Brownlow; Charles William Earl of Charleville; Alex. Crichton, M.D.; Sir Benj. Hobhouse, Bart.; Capt. Henry Kater; Daniel Moore, Esq.; Right Hon. Sir John Nicholl, Knt.; the Rev. Thomas Rackett, M.A.; and the Right Hon. C. Yorke.

Dec. 9.—A paper, by J. F. Herschell, Esq. F.R.S. was begun. It was entitled "On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints which many of them develope on Exposure to a polarized Ray."

Dec. 16 and 23.—This elaborate paper was continued.

LINNÆAN SOCIETY.

This Society commenced its meetings on Nov. 2, when a paper, by Henry Thomas Colebrooke, Esq. F.L.S. was begun. It was entitled "On the Waltiedde and Memispermum Fenestratum of Gœrtner, and divers Menisperma described by Roxburgh."

Nov. 16.—The above paper was continued.

Dec. 7.—There were read some Observations on Buxbaumia Aphylla, in a letter from Mr. J. Stewart, Lecturer on Botany in Edinburgh to Sir Joseph Banks.

Dec. 21.—Part of a paper, by —— Temminck, Esq. was read, entitled "A Description of some new Birds from Newfoundland, in the Society's Museum."

GEOLOGICAL SOCIETY.

June 18.—Some additional remarks by H. I. de la Beche, Esq. were read.

In a former paper, M. de la Beche gave an account of the fossil animal found in the blue lias of Lyme, usually called the ichthyosaurus, but which has been lately named by Sir E. Home the proteosaurus; the object of these additional remarks is to point out several species of proteosaurus which have been discovered. These are three: the communis, the tenuirostris, and the platyodon. The characters of the species are taken from the form of the teeth and the jaw bone, and the names of the two latter are derived from the shape of these parts. There are probably other species, but they are not yet sufficiently well ascertained.

The author gives, from Dr. Leach, a scientific description of the Dapedium politum, a fish with rectangular scales, that has been discovered in the lias of Lyme.

Nov. 5.—The reading of Mr. Weaver's paper " On the Geological Relations of the Environs of Tortworth, and the Mendip Range in Somersetshire," was continued.

Nov. 19.—A paper, from Dr. Nugent, was read, entitled " Sketch of the Geology of Antigua." It was accompanied by a map, sections, and specimens.

The author observes that Antigua contains no marks of modern volcanic action; but many effects of former revolutions. The south and east parts of the island exhibit recent beds of a peculiar calcareous formation, probably cotemporary with those around Paris, and in the Isle of Wight; the surface of these calcareous beds is in the form of rounded hills, like those of the chalk district of England, the highest being about 300 or 400 feet above the ocean. The materials of this formation are not uniform; a great part of it consists of closely compacted marl, of a bright yellow colour. Through this marl run layers of compact limestone containing shells, calcareous spar, quartz, chalcedony, and agate. Strata of gritstone also run through the marl, composed of quartz, hornblende, jasper, hornstone, and green earth, held together by an argillaceous cement; this is used in masonry. The marl also contains a smooth-grained calcareous sandstone, which is employed for building. The calcareous formation contains many shells and corallines, both calcareous and silicified; most of them are analogous to those in the neighbouring sea, but it is probable that they have been deposited in their present situation at a remote period.

The marl contains very great quantities of a substance called ground pearl, the nature of which is not well ascertained. The marl also contains both marine and fresh water remains, but they are mixed together; the silicified and agatized corallines, which are so abundant and so beautiful in Antigua, are very plentiful in the calcareous beds; they contain no remains of large animals, and no gypsum. Below the calcareous formation, and lying south of it, are extensive irregular masses of coarse flint or chert. They contain a large quantity of shells, chiefly cerithea, filled

with chalcedony; the inferior part of these beds contains a very
large quantity of petrified wood. Below the marl and chert is a
series of stratified rocks which the author calls claystone con-
glomerate; they compose hills that are precipitous on one side,
and slope gradually on the other. In small specimens, the rock
resembles a clay porphyry, but it has not the usual relations of
this rock. It dips at a considerable angle to the north-west; it
contains so much chlorite as to exhibit a green tinge; this is
commonly thought to depend upon copper; but the author attri-
butes it either to iron or manganese. The rock acquires a
conglomerate appearance from the numerous specimens of petri-
fied wood and fossils of all kinds which it contains.

The woods are all tropical species, and generally of the palm
tribe. The most elevated parts of the island consist of rocks of
the newest floetz trap formation; but the author conceives that
they are composed of a very large proportion of boulders included
in the conglomerate.

A communication was read from Dr. Gmelin, of Tubingen, on
the subject of the clinkstone of Hohentwiel, upon which natrolite
is found. He informs us, that by heating it, he obtained from it
a quantity of ammonia; and he found the same results from some
basalts which he tried.

ROYAL ACADEMY OF SCIENCES AT PARIS.

*An Analysis of the Labours of the Royal Academy of Sciences
during the Year* 1818.

(*Continued from* vol. xiv. p. 229.)

*An Historical Essay upon the Services and Scientific Works of
Gaspard Monge; by M. C. Dupin, a pupil of Monge, and mem-
ber of the French Institute.—" G. Monge was born at Beaune
in 1746. His progress was such that they gave him the office of
Professor of Natural Philosophy in the college at Lyons although
he had only begun to study it the year before.....Returning to
Beaune in the vacation, he set about the survey of that town.
As he had not proper instruments for that purpose, he made
some himself. He dedicated his work to the administration of
his native place, and they recompensed the young author, as far
as the limited finances of the place would allow. A Lieutenant-
Colonel of the Engineers, who happened to be at Beaune,
obtained for Monge an appointment as draughtsman and pupil
in the Ecole d'Apparailleurs et de Conducteurs des Travaux des
Fortifications (equivalent to our Drawing School in the Tower).
As he was an excellent draughtsman, his manual dexterity was
alone considered. He, however, already knew his own strength,
and saw with great indignation the value that was exclusively
bestowed on his mechanical talents. ' I was tempted,' said he,
' a long time afterwards, a thousand times, to tear my drawings,
out of spite for the value set on them, just as if I had been good

for nothing else.' The director of the school ordered him to calculate a particular case of *defilement*, an operation in which the relief and groundwork of fortifications is to be combined together with the smallest possible charge, but so that the defenders may be sheltered from the shot of the assailants. Monge abandoned the method hitherto followed, and discovered the first general geometrical method that was known for this important operation. ....By applying, at different times, his mathematical talents to questions of a similar nature, and always generalizing his manner of conceiving and working them, he, at last, formed a scientific work on the subject; this was his Descriptive Geometry. ....For more than 20 years, he found it impossible to show to the corps stationed at Mezieres the application of his geometry to carpentry. He was more successful in its application to masonry; he studied with great care the methods hitherto employed, and simplifying them he brought them to perfection by his geometry."

"His scientific works caused him to be appointed Acting Professor of the Mathematics and Natural Philosophy, in the room of Nollet and Bossut; afterwards he was appointed Honorary Professor: he then turned his views towards the study of many phenomena of nature; he made numerous experiments upon electricity; he explained the phenomena which arise from capillary attraction; was the creator of an ingenious system of meteorology; he examined the composition of water, having made that great discovery without having any knowledge of the experiments which had just before been made by Lavoisier, Laplace, and Cavendish. He did not content himself with explaining to his pupils in the theatre of the school the theories of science and their application: he loved to conduct his disciples wherever the phenomena of nature, or the works of art, could render these applications apparent and interesting. He communicated his own ardour and enthusiasm to his pupils, and changed those observations and researches into desirable pleasures, which would have appeared to be a disagreeable study in the confinement of a school, and clothed only in abstract ideas."

"In order to bring Monge to Paris, he was appointed in 1780 assistant to Bossut, Professor of the Hydrodynamic Course, instituted by Turgot. That he might reconcile the duty of the two places which he now held, he lived six months at Mezieres, and six months at Paris. The same year he was admitted into the Academy of Sciences; and on the death of Bezout in 1783, he was chosen to succeed that celebrated examiner of the naval service. The Marquis de Castries invited Monge several times to write another elementary course of the mathematics for the youths of the naval service, but Monge always refused to comply. Bezout, said he, has left a widow with no other fortune than her late husband's works, and I do not wish to take away the bread from the widow of one who has rendered important services to

science and to his country. The only elementary work which
Monge published was his Traité de Statique; and, with the
exception of a few passages in which greater rigour might be
desirable, the Statique of Monge is a model of logic, clearness,
and simplicity."

"At that period when the public distress called forth all the
useful talents and courage of the superior classes to the assist-
ance of their country menaced with invasion, Monge was created
Minister of the Marine. He did every thing he could to keep
those men who were distinguished for their merit or bravery in
France. He even descended to entreaties to procure the conti-
nuation of Borda's services, and he had the happiness to succeed.
He was one of the most active men in those scientific services
which the preservation of the state required. The construction
of the new grinding machines erected in the powder mills at
Grenoble was his, and also the drilling machines constructed
upon the barges of the Seine. He spent his days in giving
instructions and superintending the workmen, and his nights in
writing his treatise on the casting of artillery, a work designed
for the use of directors of foundries, and for workmen."

"It was in his course at the Normal School that he first gave
his lectures of descriptive geometry, the secrets of which he had
not been able to reveal sooner. Another establishment, which
had been originally conceived before the Normal School, but
which, having had more attention paid to it by the inventors,
followed it in the order of execution, realized some part of the
hopes which had been looked for in vain on the establishment
of the first Encyclopedic School that had been opened in France.
Monge brought into it his long experience at Mezieres, and
joined to this new and profound views; he drew up the plan of
study, marked out their succession, and proposed scientific
methods of execution. Out of 400 pupils originally placed in
the Polytechnic School, 50 of the choicest were collected into a
preparatory school. Monge was almost the only one that taught
these pupils. He remained the whole of the day among them,
giving them, in turn, lectures on geometry and analysis ......
exhorting them, encouraging them, inflaming them, with that
ardour, that kindness, that impetuosity of genius, which made
him explain to these pupils the truths of science with an irresist-
ible force and charm. In the evening, when these labours were
finished, Monge began others of a different kind; he wrote the
sketches which were to serve as a text to his next lectures, and
the next day he was to be found along with his pupils at the very
moment of their meeting. The good nature of Monge was
neither the cold calculation of the sage, nor even the effect of
education; it was a simple benevolence which arose from his
happy organization. He was born to love and to admire. His
admiration was excessive like his love; in consequence of which
he did not always keep within the limits that cold and unfeeling

reason would have prescribed. ....As he was the father of his pupils in the school, so he was in camp the father of the soldier."

" In traversing Italy to collect the statues and pictures that had been ceded to France, Monge was struck with the singular contrast between the Grecian monuments of the arts and those of the Egyptians, transported by Augustus and his successors to the shores of the Tiber. The comparative characters of the ancient monuments were the frequent subject of conversation between the conqueror of Italy and the commissary who collected for his country the most precious fruits of victory. Monge conceived the idea of extending the domain of history beyond the fabulous ages of Greece; of learning with the certainty of a geometer what were the labours of the ancient sages of the East; of discovering afresh, by the contemplation of their monuments, what had been the processes of their arts, the usages of their public life, the order and the majesty of their feasts, and of their ceremonies."

" Monge, charged by the General in Chief to carry to the Directory the treaty of Campo Formio, was a short time afterwards placed in the first rank of the literary men who composed the Commission of Sciences and Arts which were to accompany the expedition to Egypt. He was the first that was appointed President of the Institute of Egypt formed on the model of the French Institute. He visited the pyramids twice, he saw the obelisk and the grand ruins of Heliopolis, he studied the remains of antiquity scattered round Cairo and Alexandria. It was during a tedious march in the middle of the desert that he discovered the cause of that wonderful phenomenon known by the name of *mirage*. At the time of the revolt of Cairo, there were in the city only a few detachments of the troops. The palace of the Institute was guarded by the members themselves; and it was proposed to sally out and join the main guard; but Monge and Berthollet, considering that the palace contained the books, manuscripts, plans, and antiquities, which were the fruits of the expedition, maintained that it was the duty of the members to guard this precious deposit, and that they ought to defend that treasure at the hazard of their lives."

" Monge presided in the Commission of the Sciences and the Arts in Egypt; he contributed by his councils to form that wise plan, and by arranging and proportioning the various parts endeavoured to execute it in the utmost perfection."

" Monge had an inimitable method of exposing the most abstract truths, and of rendering them plain by the language of action. Nevertheless it was only by combating with nature that he was able to become an excellent professor: he spoke with difficulty, and almost stammered; the prosody of his discourse was vicious, for he lengthened some syllables falsely, and shortened others. His physiognomy, naturally calm, exhibited the

appearance of meditation ; but as soon as he spoke, he appeared quite another man ; his eyes acquired a sudden brilliancy ; his countenance became animated, and his figure seemed as if inspired......"

 " Monge, debilitated by age, was at last the victim of an imagination which, according as the times were adverse or prosperous, carried him beyond either just fears or just hopes...... His last moments were without last thoughts—without last effusions—without any adieu : he sunk in silence—without agonies—without terror—and without hopes......The rules of the service did not allow the generous youths to deposit at the time of his funeral the token of their remembrance and their regret upon the tomb of their old benefactor ; but on the dawn of the day after the funeral, the pupils went silently to the place of burial, and fixed upon it an oaken bough, to which they hung a crown of laurel. Twenty-three former pupils of the Polytechnic School, residing in the town of Douay, joined together with one accord, and wrote to M. Berthollet to beg he would superintend the erection of a monument to be built at the expense of the former pupils of the Polytechnic School, in honour of Gaspard Monge. M. Bertrand, Notary, No. 46, Rue Coquillière, Paris, is charged with the receipt of the subscriptions. The pupils who have studied architecture are invited to propose their plans for Monge's monument, and to send them, with an estimate, to M. Bertrand."

This notice is terminated by a list of pupils who have already subscribed. The second part contains a catalogue and analytical review of Monge's writings, not only of those which he published separately, but also of those which are inserted in the Memoirs of the Academy, or of the Polytechnic School, and in many other collections. All these works are well known, and duly appreciated ; we have extracted in preference the slighter anecdotes, those which, exhibiting as it were the mind of Monge, explain the attachment of his former pupils, and the regrets of his fellow labourers.

On the Pontine Marshes; by M. De Prony. Paris, 1818.— At the meeting of Jan. 9, 1815, the author read a memoir, in which he gave a general idea of the great problem relative to draining and rendering wholesome the Pontine Marshes. This memoir appears again at the head of this work, of which it forms the introductory part, and is accompanied with interesting notes, which could not be got into the text.

In the 442d year of Rome, at the time of the construction of the Appian way, the Pontine district was in a marshy state. About 150 years afterwards, Cornelius Cethegus undertook the draining of it. These works were afterwards neglected until the dictatorship of Julius Cæsar, whose vast projects were interrupted by his death. Nero, Trajan, and their successors, bestowed much attention on the Appian way, and but little on

the Pontine Marshes. Theodoric confided the draining of it to Decius. Leo X. and Sextus V. caused several works worthy of notice to be executed, but these were by no means to be compared with the works executed from 1777 to 1796 under the pontificate of Pius VI. who expended 9,000,000 of francs upon them. Unfortunately, the plan had been laid on theoretical views, very specious, and very seducing, proper indeed in many respects, but being too general, the consequences were unfortunate ; so that these works considered in a hydraulic point of view exhibit mere sketches of vast conceptions, in which many parts of great importance are wanting, having been thought unworthy of notice. Very circumstantial, historical, and critical details upon all these objects are to be found in this work.

By means of borers, it has been ascertained that the sea formerly washed the feet of the mountains which form the eastern and southern boundaries of the Pontine Marshes. The whole of the phenomena concur in showing that the formation of these marshes was caused, on the one hand, by brooks and torrents running into a gulph which formerly covered the isles of Circe, Zanoni, and Ponza ; and, on the other hand, by the sea forming two ridges of sand banks, the last of which has at length shut up the communication between the sea and the internal gulph. Colmates's method of employing currents of water charged with mud to raise up the soil by its deposition and successive increase, offers here only a secondary resource, as its effect is so very slow. Notwithstanding its insufficiency, it will be proper to continue the trials of this kind, which have been already begun. And M. de Prony advises the use of it for the amelioration of some soils, to which he believes it to be very applicable ; but it cannot be considered as any other than a subsidiary means. The principal method to be used for the draining of these marshes can only be a good system of canals to carry off the water. To establish a system of this kind, it would be necessary, in the first instance, to procure an exact plan of the ground, its declivities, its torrents, its rivulets, the quantity of rain that falls annually on it, and the quantity it throws off by evaporation. This preliminary knowledge, being at present very imperfect, M. de Prony has begun to fill up the parts that were still wanting to complete it. By means of three signals placed at known distances on the same straight line, he determined, in the most expeditious manner, all the points from whence he could observe his three signals. He has also reduced this curious problem to general and convenient formulæ. In fact, this is only a particular case of the problem by which Hipparchus determined the eccentricity and distances of the sun and moon. Snell was the first that transferred this problem from the heavens to the earth, and made use of it in surveying. Hipparchus's problem has been reduced by us into general formulæ, which comprehend the case used by M. Prony. We have been curious

enough to compare the two methods; and we have found them equally exact, and equally expeditious.

Thus, by uniting together the different memoirs which have been communicated to him, along with the results of his own survey, and the observations he made during his residence in the Pontine Marshes; as also with the levels, borings, and other works, executed, at his desire, by that skilful engineer M. Scaccia, the author has been able to form, for his direction in the project of draining these marshes, a collection of materials much more complete than those on which the former projects had been undertaken. The recent progress of the doctrines of running water has also furnished him with means which were wanting to his predecessors. By the help of all these means, he has been able to form a plan which will satisfy all the conditions required by that celebrated problem, the draining of these marshes.

The work is divided into four sections. The first contains the description and dimensions of the Pontine basin; the second contains the state of these marshes before the execution of the works ordered by Pius VI. In the third, there is given a description of their present state, and an analysis of the different projects formed anterior to 1811. The fourth and last contains the author's own views, and his projects for the ulterior benefit of the Pontine Marshes. In all of these we find a number of curious and instructive tables, in which the author has collected all the results of his observations and calculations. It is evident that it is impossible for us to analyze them; we shall only notice the true measure of the ancient Roman foot, deduced from the distance of the 42d and 46th milestone on the Appian way, the only ones which have not been thrown down and removed. The true Roman foot is 0·294246 metre, or 10 inches 10 lines ·044 of the old Paris foot.

In the fourth section, which is the most extensive, and contains more especially the application of the hydraulic theories, the evil consequences of paring and burning the soil are shown; also the present state of the Po, which, by means of the soil it deposits at its mouth, now gains 70 metres yearly from the sea, instead of 25, which it gained yearly from the 12th to the 17th century; the deposition of soil formed by the Tiber; and lastly, the contrary effect produced by the sea on the shore between Anzo and Astura. "The Italians are, perhaps, the first who gave to modern Europe the example of moderating the descent and velocity of currents by means of falls, but they never employed sluices for any other purpose; the glory of using sluices to establish a communication between two large basins was reserved for France. The canal of Briare, which joins the Loire to the Seine, and which was finished in 1642, is the first example of the union of two rivers by a canal traversing the country lying between their two beds. This example was followed with great success by the contrivers of the canal of

Languedoc, begun in 1668, and finished in 1681. Thus the French engineers have, without the least contradiction, the exclusive glory of having invented canals, and of having produced, as the first application of this invention, two grand works, justly esteemed the finest in their kind; and the novelty of which has not been sufficiently remarked or perceived by those who have written the history of the art."

The remainder of the work contains, at full length, the application of the principles and formulæ given by the author in his Physico-mathematical Researches upon Running Waters. These principles and methods, established upon all the experiments the author could collect, have been already reduced to practice in several cases, and especially in a very large drainage; namely, that of the marshes of Bourgoin directed by M. Roland.

The whole review of this work shows this important consequence—the possibility of including in regular canals all the water which inundates this unfortunate soil, and of giving it a free and easy passage to the sea. The draining being completed by the means now pointed out, the keeping the ground in good culture will be neither difficult nor expensive, but it should be followed up with great care.

The results of such extensive researches for rendering wholesome the neighbourhood of Rome, and for its prosperity, cannot be foreseen at present with much certainty: the author has done every thing that depended on him; and his work will at least exhibit to young engineers an useful example of the union of theory and experience in forming the project of an extensive drainage.

ARTICLE IX.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Potatoe.*

The general opinion is, that the potatoe is indigenous in America, and that it was brought from that continent to Europe by the Spaniards soon after the discovery of America by Columbus. A fact mentioned in the Transactions of the Linnæan Society, vol. xii. p. 585, may, perhaps, be considered as a corroboration of this opinion. Don Jose Pavon, of Madrid, one of the authors of the *Flora Peruviana*, states, in a letter to Mr. Lambert, that he and his companions Ruiz and Dombey had found the potatoe (*Solanum tuberosum*) growing wild in the environs of Lima, and 14 leagues from thence on the coast of

Peru, as well as in Chili; and that it is cultivated very abundantly in those countries by the Indians, who call it Papas.

II. *Remarkable Difference between the Celestial and Terrestrial Arc of the Meridian.*

In the year 1808, Baron von Zach determined the latitude of Florence at the Observatory of the Grand Ducal Observatory, and found it by 506 observations of different stars to be 43° 46′ 4·31″. In 1809, he determined the latitude of Pisa by 504 observations, made at the Observatory, and found it to be 43° 43′ 11·77″; so that the difference between the latitude of these two cities is 2′ 52·54″.

Baron von Zach likewise ascertained the longitudes of these two places, by observations of the occultations of various stars by the moon, and found them as follows:

Florence, 35′ 40·2″ in time E. of the Observatory of Paris
Pisa,　　　32　8·0

In the year 1815, when Tuscany was restored to its legitimate sovereign, Ferdinand IV. the reigning Grand Duke ordered a trigonometrical survey of the whole Grand Duchy to be made. This survey was committed to the care of Father Inghirami, Professor of Astronomy, and Director of the two Observatories of Florence. He conducted it with the greatest possible precision, as may be seen in the three following memoirs, which he published in succession.

1. Dalla Longitudine e Latitudine delle Citta di Pistoja, e di Prato. Estratta dal vol. delle Memorie di Scienze Matem. e Fisic. dell' Imp. e R. Accademia Pistojese, per l'Anno 1816. Pistoja, 1816.

2. Della Longitudine et Latitudine Geografica della Citta di Volterra, S. Miniato e Fiesole. Firenze, 1817.

3. Di una Base Trigonometrica Misurata in Toscano nell' autunno del 1817, letta in Livorna all' Accademia Labronica il di 7 Febrajo, 1818. Firenze, 1818.

During the course of this survey, he joined Florence and Pisa together by a series of triangles, and thus was enabled to determine geometrically the difference between their latitudes. Baron von Zach had determined the latitude of Pisa astronomically,

```
                                    43°  43′  11·77″
Inghirami found it ........ 43   43   19·40
                           ─────────────────
     Difference................ 00   00    7·63
```

This enormous difference, if we were to ascribe it to the operations of Inghirami, would suppose an error of at least 100 toises in the geodesiacal operations; which is quite inadmissible.

The longitude of Pisa, as found by the astronomical observations and the trigonometrical survey, was likewise different.

Zach found it... 28° 2' 0·0" E. from Ferro
Inghirami 28   4   1·2
                  ─────────────
Difference 00   2   1·2

This difference, though much greater than the former, is not
so surprising.

Inghirami was much mortified at these discrepancies, which he
considered as throwing a slur upon the accuracy of his opera-
tions. He, therefore, used every possible exertion to get rid of
them. At first, he rested satisfied with a short base which
Baron von Zach had measured during his observations, to deter-
mine the latitude of Florence; but he afterwards measured a
much longer one himself, and went over all the parts of his
calculations again and again with the most scrupulous attention;
but his efforts were unavailing. Baron von Zach considers the
error to lie in the astronomical observations. He is of opinion
that astronomical instruments, notwithstanding the great accu-
racy with which they are now made, are not capable of giving
the true latitude nearer than two or three seconds. Another
cause of the difference, he thinks, may be the attraction of
mountains, or of masses of matter below the surface of the earth,
having a greater or smaller specific gravity than usual, which,
by acting on the plumb line, must occasion an error in the
astronomical observations. A third cause, he conceives to be
the irregularity of the figure of the conformation of the earth.—
(Correspondence Astronomique, Geographique, Hydrographique
et Statistique, du Baron de Zach, vol. i. p. 1. Genoa, 1818.)

III. *Positions of different Places on the North Coast of Africa.*

Capt. William Henry Smyth, of Royal Navy, at present
employed in the Mediterranean in an astronomical, geographical,
and hydrographical mission, has determined the latitudes and
longitudes of the following places on the north coast of Africa as
follows:

	North latitude.			Longitude East from Ferro.		
Tripoli (house of the British Consul)	32°	54'	13"	30°	50'	30"
Leptis magna (tent in the ruins) . . .	32	37	23	31	47	15
Gherrza (at the tombs)	30	37	30	31	48	30
Cyniphus (river, mouth of)	32	33	25	31	54	20
Asal Amrah (the point)	32	51	5	31	30	0
Benhouleat (the castle)	31	28	10	30	58	0
Cape Bon (the tower)	37	4	45	28	43	45
Pantellaria island (the castle)	29	35	5	36	51	15
Tajoura (the point)	32	53	0	31	1	18
Galita island (the centre)	37	32	30	26	34	50

(Ibid. p. 68.)

IV. *Position of different Places in Sicily and the Neighbourhood, determined by Capt. W. H. Smyth.*

	North latitude.			Longitude East from Ferro.		
Messina (the fanal)	38°	11′	30″	33°	14′	55″
Syracuse (the fanal)	37	2	58	32	55	55
Milazzo (the fanal, Cape Bianco). ..	38	15	45	32	54	15
Cape St. Vito.	38	10	50	30	26	0
Cape de Gallo (Bay of Palermo) ...	38	14	10	31	1	30
Ustica Isle (Fort Falconara)	38	43	40	30	51	45
Stromboli Isle (church of St. Bartolo)	38	48	0	32	52	55
Lipari Isle (castle).	38	28	35	32	35	25
Felicuri Isle (church)	38	34	15	32	5	55
Maritimo Isle (castle)	38	1	10	29	43	40

(Ibid.)

V. *Positions of different Places on the North Coast of Africa, determined by Don Dionysio Alcala Galiano.*

The best chart of the Mediterranean, which has yet appeared, was published by the Spaniards, in the year 1804, under the following title : " Carta esferica que comprehende las costas de Italia, las del Mar Adriatico, des de Cabo Venere hasta las islas, Sapiencie en la Morea, y las correspondientes de Africa, parte de las islas de Concega, y Cerdena, con las demas que comprehende este mar ec. Corregida la costa de Africa y las islas de Sicilia, de Lipari y Sapiencie en 1804, por las observationes ec. por D. Dionysio Alcala Galiano ec."

The latitudes and longitudes in that chart correspond well with those given by Captain Smyth. Hence there is every reason to trust in the precision of the following places on the northern coast of Africa, taken from Galiano's chart:

	North latitude.			Longitude East from Ferro.		
Isola Piana.	37°	1′	30″	28°	8′	30″
Cape Blanco	37	17	0	27	40	30
Isles of Dogs.	37	18	0	27	56	30
Cape Zebibi	37	15	0	28	4	30
Porto Cartago	36	52	0	28	9	30
Goletta de Tunis	36	48	30	28	5	30
Iles d'Imbres.	37	7	0	28	37	30
Isle Lampione	35	35	0	30	10	30
Isle Linosa.	35	50	30	30	38	30

The following positions of places on the coast of Sicily are likewise taken from the same chart:

	North latitude.	Longitude East from Ferro.
Ile Salina	38° 33′ 30″	38° 26′ 0″
Torre del Faro	38 15 30	33 21 30
Agosta	37 17 30	
Cap de Morro	37 2 0	
Cap di Passaro	36 39 30	
Cap Scaramic	36 46 30	
Punta della Saeta...............	37 51 30	33 27 00

(Ibid. p. 73.)

VI. *Position of various Places in the Mediterranean by Mr. Rumker.*

	North latitude.	Long. East from Greenwich.	Variation of the compass.
Corfu.	39° 36′ 30″	19° 32′ 15″	
S. Vito, near Corfu	39 37 25		
S. Maura..............	38 40 00	20 35 00	
Isle of Ithaca	38 12 00	20 35 00	
Port Chieri, in Zante	37 46 00	20 55 15	
Syracuse..	37 2 00	15 13 15	
Isle Maritimo	38 1 00	12 2 00	17° W.
Isle Gorgonna..........	43 26 00	10 9 00	19
Cagliari, in Sardinia.....	39 12 00		
Isle Cabrera, below Majorca	39 7 00	1 40 15	
Isle Alboran (Africa)	35 56 00	3 1 0	
Gibraltar (European point)	36 5 00	5 10 W	

(Ibid. p. 76.)

VII. *Position of La Valetta, in Malta.*

Baron von Zach has fixed the latitude and longitude of this place as follows, from the observations of Father Feuillé:

North latitude, 35° 54′ 30″

East longitude, 32   5   53   from Ferro, or 48′ 23·5″ in time east of Paris.—(Ibid. p. 83.)

VIII. *Geographical Positions of several Places in France, Switzerland, and Germany.*

Places.	Latitude.			Longitude E. from Ferro.			Longitude E. of Paris.		Height of French feet.
Strasburgh......................	48°	34′	57·60″	25°	25′	16·46″	21′	41·1″	448·5
Donon........................	48	30	48·34	24	50	6·25	19	20·4	3108·6
Bressoir......................	48	11	25·20	24	49	13·86	19	16·9	3788·5
Balon	47	54	5·39	24	46	7·89	19	4·5	4401·3
Bolchenberg	47	49	23·52	25	30	12·66	22	0 8	4356·6
Colmar.......................	48	4	41·22	25	1	42·33	20	6·8	
Oberberghen ⎱ Term of the	47	57	44·85	25	4	18·63	20	17·3	652·3
Saursheim ⎰ base.	47	47	29·02	25	3	29·09	20	13·9	720·7
Rothifluh...................	47	15	30·81	25	11	54·79	20	47·6	1399·6
Chasseral....................	47	8	0·87	24	43	46·98	18	55·1	4960·6
Berne........................	46	56	51·91	25	7	18·39	20	29·2	1646·3
Walperswyl ⎱ Term of the	47	3	23·69	24	54	17·57	19	37·2	1377·2
Sugy........⎰ base.	46	57	46·57	24	48	2·54	19	12·2	1947·1
Lichtenberg.................	48	55	19·50	25	9	27·95	20	37·9	1278·5
Rastadt......................	48	51	29·23	25	52	26·83	23	29·8	506·4
Seberkole....................	49	1	43·24	25	33	25·02	22	13·7	1559·9
Michelsberg.................	49	5	20·24	26	13	56·90	24	55·8	796·1
Calmet......................	49	19	12·39	25	45	13·51	23	0·9	2077·0
Mannheim, *Observat.*........	49	29	15·13	26	7	51·72	24	31·5	302·3
Mont-Tonnere...............	49	·37	25·86	25	36	10·30	22	24·7	2090·0
Perberg	49	31	19·32	25	9	3·78	20	36·3	1736·0
Melibocus.............. ...	49	43	32·82	26	18	27·64	25	13·8	1677·4
Merstein....................	49	52	45·92	25	59	52·88	23	59·5	
Darmstadt (*Base*)	49	52	20·94	26	19	39·34	25	18·6	448·8
Nierstein...................	49	51	39·53	26	13	16·40	24	53·1	594·5
Frankfort, s. M.	50	6	42·49	26	21	23·28	25	25·5	

The above table is published in the Correspondence Astronomique, Geographique, Hydrographique et Statistique, of Baron von Zach, vol. i. p. 149. The positions were ascertained by geographical engineers, of the Bureau Topographique at Paris, some of them nearly 20 years ago, but the results had not been published before. Baron von Zach considers it as exceedingly correct.

IX. *Latitude of the Observatory at Manheim.*

Prof. Schumacher, at Copenhagen, from a great number of observations, has determined the latitude of the Observatory at Manheim to be 49° 29′ 13·70″.—(See Zach's Corr. Astron. i. 193.)

X. *Height of the Passage over the Splugen.*

The Austrian engineers at present employed in making this road have determined the height trigonometrically, and found it 6393 French feet above the level of the sea. Dr. Schouw, one of the philosophers at present travelling at the expense of the King of Denmark, found the height, by a barometrical observation, 6451 French feet. The difference between these two determinations is only 58 feet, which, in the case of an isolated

barometrical observation, may be considered very small. Formerly the height was reckoned only 5926 French feet. This is the height given in the Almanack of Gotha.—(Ibid. p. 197.)

XI. *Further Observations on the Double Rainbow seen at Paisley.* By Mr. Macome.

(To Dr. Thomson.)

DEAR SIR,   *Paisley, Nov.* 16, 1819.

The kindness with which you gratified me with a solution of such difficulties in chemistry as opposed my attempts to acquire some knowledge of that fascinating science, induced me to trouble you with a notice of a singular rainbow I had lately witnessed, and in which the more refrangible colouring rays were distinctly repeated—an appearance which I could not account for. As the statement, however, was drawn up without the most distant view towards publication, I perceive it is not sufficiently explicit in detailing the principal attendant circumstance—a knowledge of which may probably lead to an explanation of the phenomenon. While viewing a rainbow of the usual appearance, a driving shower from the south-west was moving in a direction which would cross obliquely a line uniting me with the southern limb of the arch. When the shower approached this line, the back ground, on which that beautiful painting of nature was delineated, darkened exceedingly, and exhibited a quadrant, the most brilliant I had ever beheld, with the following number and arrangement of the colours : red, orange, yellow, green, blue, violet, green, blue, and violet. It was the arrangement of the last three mentioned colours that puzzled me ; for had the order been reversed, I should have been disposed to refer their production to a partially reflected image of the usual spectrum. But this was by no means the case, nor did any fainter shade of colouring indicate an origin different from that of their less equivocal neighbours. The encroachments of the advancing shower left me little opportunity of further observation, only an impression remained on my mind that the breadth of the whole spectrum was not proportional to what might have been expected from the great refrangibility of the rays which formed this novel addition to it.   I am, dear Sir, your obedient servant,

A. MACOME.

XII. *On Rain-Gauges.* By Mr. Holt.

(To Dr. Thomson.)

DEAR SIR,   *Cork, Oct.* 9, 1819.

In the Transactions of the Wernerian Natural History Society for December, 1815, Mr. Kerr laid before the Society a model and description of a new rain-gauge, constructed on the principles which seem to have actuated M. Flaugergues to assert in the 8th volume of the Bibliotheque Universelle (and thence trans-

lated into the *Annals of Philosophy* for August), that it was
not difficult to account for the difference in the quantities of
rain that fall into rain-gauges placed at different heights.
Your correspondent Mr. Meickle, in the *Annals of Philosophy* for
September, objects to the theory of Mr. Kerr and M. Flau-
gergues as unphilosophical; and endeavours, by a mathematical
diagram, to show that, be the inclination of the rain what it may,
the same quantity of drops will be received by the rain-gauge.
It appears to me Mr. Meickle has taken a wrong view of the
subject. I shall endeavour to express my ideas in as few words
as possible. If the receiving surface of the rain-gauge be dis-
posed parallel to the horizon, it may be readily shown that
should the rain fall perpendicularly, it will receive its due propor-
tion; but it may also be as readily shown that if the rain be
driven by a current so as to fall in an angle more or less inclined
to the horizon, the same quantity of rain can never enter the
gauge. Should the rain be blown in a direction parallel to the
horizon, it is obvious *none* could enter: if the inclination be at
10°, the quantity received will be less than if the angle were 20°;
much less than at 30°; and so on: so that the quantity received
will be always in proportion to the angle of inclination. The
annexed diagram will illustrate
the subject. Let *a, b, c, d,*
represent the vertical section of
the common rain-gauge; *a c* or
b d perpendicular to the hori-
zon. If the rain be inclined at
45°, as at *e a, f c,* the quantity
which can enter through *a b*
will be as the rectangle con-
tained by *e g,* perpendicular to
e a or *f c,* and the side *a b,*
which rectangle is equal to about seven-tenths of the square of
a b. If the inclination equal 20°, as at *h a, i k,* the quantity
received will be as the rectangle under *l m* and *a b;* equal to
about one-third of the square of *a b.* If the inclination be 60°,
as at *n a, o p,* the receiving superficies will be equal to the
rectangle under *n q* and *a b,* about $\frac{17}{20}$ of the square of *a b.* Thus
it is evident that in no inclination can so great a quantity of rain
enter as when it falls perpendicularly.
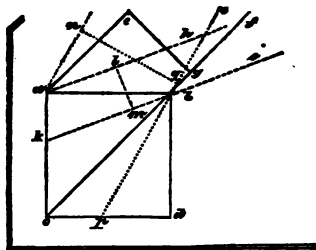
Perhaps the phenomenon of more rain being collected in a
low confined situation than in an exposed high one may be
explained thus : a current of wind, impinging the flat side *a b c d*
of the gauge, would be glanced round the edges *a c* and *b d,*
and also over the top *a b,* it would, therefore, sweep off a consi-
derable portion of the falling rain, and thereby induce a false
estimate; the same cause operating in a much less degree in a
low or confined situation would necessarily occasion the com-

monly observed results. Should we adopt the gauge recommended by Mr. Kerr, it would be necessary to construct a scale of the surface at every degree of the rain's inclination, as it will be observed that the maximum of surface will be when the rain inclines in an angle of 45°, and the minimum when either horizontal or vertical.

If the gauge were shaped like a funnel, it would in a great degree prevent the effect of height or exposure, or the quantity of rain collected, because the impinging current would be inclined to the smaller end of the cone; and little, if any, would be disposed to pass over the top.

<div style="text-align: center">I have the honour to be, dear Sir,

Your most obedient humble servant,

THOMAS HOLT.</div>

XIII. *Further Remarks on propelling Vessels by Windmill Sails.* By Mr. Bartlett.

<div style="text-align: center">(To Dr. Thomson.)</div>

SIR,   *Buckingham, Nov.* 1, 1819.

Permit me to point out the following *errata* which appear in a note (pp. 359, 360, of your number for the present month) to my paper On the Propulsion of Vessels by Means of Windmill Sails, which you did me the honour of copying into your *Annals*. They arise from the incorrect manner in which the decimals are pointed off, viz. 4,021,248 square feet should be 4021·248; $670,208 \times 32 = 21,446,656$ lbs. $670·208 \times 32 = 21446·656$; and 128,679,936 lbs. 128679·936; the three right hand figures of each quantity being decimal fractions.

The note was thus erroneously printed in the work which you extracted it from; but, as the product of those numbers was correct, I did not think it necessary to notice the mistake, until copied into your pages.

You remark that the idea itself " is not new," it having been " proposed nearly a century ago ; " in reply to which, I beg to observe, that it can certainly lay but little claim to *originality :* being nothing more than the application of one power to the mechanical impulsion of another; in other words, imparting momenta to the machinery of steam-vessels by means of windmill sails. But, if I am not mistaken, you allude to the plan of *Bishop Wilkins* for impelling *land carriages,* which I conceive, both in its construction and application, to be widely different from the one I propose. I did not know, until long after I had submitted my ideas upon the subject to the world in a separate treatise (which afterwards appeared in the Pamphleteer), that such a suggestion had been made as to the possibility of moving carriages on wheels by means of revolving sails ; and I have yet to learn that it had ever been proposed so to impel floating bodies.

The idea first suggested itself to me upon witnessing the

construction of a mill worked by the two powers (i. e. steam and wind); and had not the present mode of navigation by the former force been so widely diffused, it is more than probable that it never would have occurred to me.

I have the honour to be, Sir, your very obedient servant,

J. M. BARTLETT.

XIV. *Galvanic Experiment.*

(To Dr. Thomson.)

SIR,   *Glasgow, Oct. 9, 1819.*

The other evening, when reclining on my pillow in a chamber where on all sides darkness was distinctly visible, and while rubbing my eyes, an employment by the by in which the hands of the indolent are frequently engaged, I happened accidentally to thrust my thumb below the superciliary ridge, at the same time raising the upper eye-lid a little, and was somewhat astonished to perceive a fine semicircle of light, which was permanent as long as I kept my thumb in the above position. To ensure success, the room must be dark, and the nail of the thumb must be towards the eye, and press considerably on the upper eye-lid while in the act of raising it.

This luminous appearance I ascribe to galvanic agency, but probably it may be owing to some other cause. However, if the experiment is actually new, and worthy of notice, you may insert it in your magazine; if not, you will pardon me for mentioning such a trifle, and excuse the ignorance and presumption of

Your most obedient humble servant,

W. R.

⁎⁎⁎ Though the preceding fact is not new, I have inserted this notice of it for the sake of many readers who may not be much conversant with physiological investigations.—T.

XV. *Singular Substance found in a Coal Tar Apparatus.* By Mr. Garden.

In one of the condensing vessels of an apparatus erected for the distillation of coal tar, and for the purpose of exposing various bodies to the action of that substance at a boiling heat, there was found a considerable quantity of a concrete matter which had distilled over with the volatile oil. The substance in the state in which I received it was mingled with a portion of darkly coloured tar oil, from which by repose it subsided in the form of a granular-like crystalline mass; when the oil was poured off, and the remaining portion separated as much as possible by passing the solid matter between several folds of blotting paper, it was digested in alcohol slightly heated; in this way nearly the whole was dissolved. The solution upon cooling deposited an abundance of crystals of a tabular shape, still coloured by a small quantity of adhering oily matter; by repeated solutions and

crystallizations, it was obtained in the form of brilliant white scaly crystals, similar to benzoic acid, but of a more silvery lustre.

This crystallized substance exhibited the following characters: Its smell was peculiarly pungent and somewhat aromatic, unlike that of any substance with which I am acquainted.

Fusible at a temperature of 184°, and completely volatilizable at that, and even lower temperatures.

Insoluble in water. Soluble in essential and in expressed oils. Readily soluble in alcohol, from which it was again separated by the addition of water.

Fuming nitrous acid assisted by a gentle heat exerted a considerable action upon it, first changing it into a brown-coloured viscid oil, which dissolved, and upon cooling, a group of minute stelliform crystals were formed, not unlike camphoric acid.

Acetic acid, when gently heated, readily dissolved the substance, but let it fall again upon cooling.

Alkaline solutions did not appear to have any remarkable action upon it.

Its solution in alcohol neither changed the colour of litmus or of turmeric paper.

From the preceding statement, it would appear that this substance bears a strong analogy to camphor in many of its characters. Camphor is soluble in acetic acid both warm and cold, and remains in permanent solution; but this substance, when dissolved in the same acid, separates from it in the form of crystals, when the solution is suffered to cool.

A more minute investigation than that which I have yet had an opportunity of instituting would be necessary to determine the true nature of this body.

Whether it be camphor in disguise, or something else, it certainly coincides in some of its habitudes with the concrete essential oils.

372, Oxford-street, Dec. 18, 1819.   A. GARDEN.

XVI. *Death of Dr. Rutherford.*

On the   of Dec. died Daniel Rutherford, M.D. Professor of Botany in the University of Edinburgh. Dr. Rutherford was the discoverer of azote, which was first described by him in his Thesis De Aere Mephitico, published in 1772.

ARTICLE X.

Astronomical, Magnetical, and Meteorological Observations.
By Col. Beaufoy, F.R.S.
Bushey Heath, near Stanmore.
Latitude 51° 37′ 42″ North.   Longitude West in time 1′ 20·7″.

Astronomical Observations.

Nov. 6. Emersion of Jupiter's first $\{$ 6ʰ 20′ 27″ Mean Time at Bushey.
satellite................. $\}$ 6 21 48   Mean Time at Greenwich.

Magnetical Observations, 1819. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 1	8ʰ 35′	24° 32′ 07″	1ʰ 35′	24° 40′ 14″		
2	8 25	24 32 47	— —	— — —		
3	8 30	24 31 48	1 35	24 38 35		
4	8 35	24 30 58	1 20	24 41 00		
5	8 30	24 32 25	1 25	24 40 13		
6	8 30	24 32 37	1 20	24 42 05		
7	8 30	24 30 45	1 25	24 37 55		
8	8 30	24 33 00	1 25	24 39 26		
9	8 35	24 31 38	1 25	24 38 40		
10	8 35	24 33 30	1 15	24 38 31		
11	8 30	24 32 46	1 15	24 39 27		
12	8 35	24 31 45	1 15	24 40 26		
13	8 35	24 33 47	1 15	24 40 32		
14	8 35	24 44 50	1 20	24 39 51		
15	8 35	24 34 45	1 20	24 38 43		
16	8 45	24 31 42	1 20	24 38 26		
17	8 35	24 33 00	1 20	24 38 34		
18	8 35	24 32 11	1 20	24 38 40		
19	8 35	24 31 39	— —	— — —		
20	8 35	24 31 37	1 20	24 38 16		
21	8 30	24 31 32	1 20	24 36 09		
22	8 35	24 31 33	1 20	24 38 33		
23	8 35	24 31 54	1 20	24 35 36		
24	8 40	24 32 39	1 20	24 38 25		
25	8 40	24 38 48	1 20	24 39 02		
26	8 40	24 33 55	1 20	24 37 19		
27	8 40	24 33 34	1 15	24 36 32		
28	8 45	24 32 41	— —	— — —		
29	— —	— — —	— —	— — —		
30	8 35	24 34 09	1 20	24 35 31		
Mean for the Month.	8 35	24 32 42	1 21	24 38 43		

(Evening Observ. column note, printed vertically:) Owing to the shortness of the days, evening observations discontinued.

In taking the mean of the morning observations, that of the
14th is rejected, being so much in excess, for which there
was no apparent cause.   On the noon of the 21st, the needle
vibrated at intervals 15, and in the evening it rained and snowed.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six'r.
Nov.		Inches.				Feet.		
	Morn....	29·400	44°	83°	ENE		Rain	43°
1	Noon....	29·332	45	77	ENE		Cloudy	46
	Even	—	—	—	—		—	39
	Morn....	29·258	41	83	N by W		Very fine	
2	Noon....	—	—	—	—		—	46
	Even	—	—	—	—		—	34
	Morn....	29·413	36	79	WNW		Clear	
3	Noon....	29·500	44	62	NW by W		Clear	44⅓
	Even	—	—	—	—		—	34¼
	Morn....	29·485	41	77	SW by W		Cloudy	
4	Noon....	29·460	40	67	W by N		Cloudy	50
	Even	—	—	—	—		—	41
	Morn....	29·248	46	73	SW		Cloudy	
5	Noon....	29·158	51	71	SW by W		Cloudy	51½
	Even	—	—	—	—		—	43½
	Morn....	29·035	44	86	SW		Fine	
6	Noon....	29·002	50	64	SW		Cloudy	50½
	Even	—	—	—	—		—	38
	Morn....	28·942	40	76	WSW		Clear	
7	Noon....	28·983	48	65	WNW		Showery	48½
	Even	—	—	—	—		—	35
	Morn....	29·110	37	79	NE by N		Very fine	
8	Noon....	29·110	41	70	NNE		Fine	42¼
	Even	—	—	—	—		—	30
	Morn....	29·402	32	73	W		Very fine	
9	Noon....	29·368	41	62	W		Cloudy.	44½
	Even	—	—	—	—		—	37½
	Morn....	28·870	44	83	W		Foggy	
10	Noon....	28·850	48	70	W by N		Fine	49
	Even	—	—	—	—		—	42¼
	Morn....	29·129	44	84	NE		Rain	
11	Noon....	29·233	44	78	NE		Rain	44⅓
	Even	—	—	—	—		—	35
	Morn....	29·500	38	79	NNE		Fine	
12	Noon....	29·480	45	73	NE		Rain	45¼
	Even	—	—	—	—		—	40
	Morn....	29·347	41	73	NE		Showery	
13	Noon....	29·323	44	66	NE		Cloudy	45
	Even	—	—	—	—		—	40
	Morn....	29·325	40	85	NNW		Sm. rain	
14	Noon....	29·323	44	74	Var.		Cloudy	44
	Even....	—	—	—	—		—	40
	Morn....	29·312	41	83	W by S		Fog	
15	Noon....	29·256	45	78	SSW		Cloudy	45½
	Even	—	—	—	—		—	38½
	Morn....	29·063	39	86	NNW		Rain	
16	Noon....	29·000	39	79	NNW		Cloudy	40
	Even ...	—	—	—	—		—	34⅓
	Morn....	29·160	39	87	NE by E		Showery	
17	Noon....	29·300	41	85	NE by E		Rain	41½
	Even	—	—	—	—		—	37
	Morn....	29·600	38	75	ENE		Fine	
18	Noon....	29·600	40	62	ENE		Very fine	43
	Even....	—	—	—	—		—	

Meteorological Observations continued.

Month	Time	Barom.	Ther.	Hyg.	Wind	Velocity	Weather	Six's
Nov.		Inches.				Feet.		
19	Morn....	29·515	30°	75°	NE		Fine	28¼
	Noon....	—	—	—	—		—	35½
	Even	—	—	—	—		—	{30¼
20	Morn....	29·230	31	69	W by S		Cloudy	40¼
	Noon....	29·066	34	68	SW		Cloudy	
	Even	—	—	—	—		—	{33½
21	Morn....	28·628	35	84	WNW		Fine	40¼
	Noon....	28·664	40	74	NW		Cloudy	
	Even	—	—	—	—		—	{30
22	Morn....	29·018	32	76	WNW		Very fine	46½
	Noon....	29·038	35	68	NW		Clear	
	Even	—	—	—	—		—	{27½
23	Morn....	29·270	29	72	W by N		Clear	35
	Noon....	29·278	35	67	WNW		Fine	
	Even ...	—	—	—	—		—	{25
24	Morn....	29·403	28	76	NW		Clear	38½
	Noon....	29·442	37	70	NW		Clear	
	Even	—	—	—	—		—	{29
25	Morn....	29·546	30	77	NNW		Clear	38
	Noon....	29·504	38	71	SW		Very fine	
	Even	—	—	—	—		—	{30
26	Morn....	29·190	33	85	NNW		Sleet	36º
	Noon....	29·223	36	80	NW		Foggy	
	Even	—	—	—	—		—	{31
27	Morn....	29·425	33	83	NW		Snow	37½
	Noon....	29·428	37	77	W by N		Fog	
	Even	—	—	—	—		—	{30
28	Morn....	29·393	30	83	SSE		Foggy	46
	Noon....	29·300	31	82	SSW		Foggy	
	Even	—	—	—	—		—	{46
29	Morn....	29·112	46	97	SSW		Rain, fog	51
	Noon....	29·135	50	94	SSW		Rain, fog	
	Even	—	—	—	—		—	{48
30	Morn....	29·220	49	94	SSW		Rain	50º
	Noon....	29·135	54	87	SSW		Showery	
	Even	—	—	—	—		—	

Rain, by the pluviameter, between noon the 1st of November, and noon the 1st of December, 1·761 inch. The quantity that fell on the roof of my Observatory, during the same period, 1·692 inch. Evaporation, between noon the 1st of November and noon the 1st of December, 1·230 inch.

ARTICLE XI.

METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER. Max.	Min.	THERMOMETER. Max.	Min.	Evap.	Rain.	Hygr. at 9 a. m.	
11th Mo.									
Nov. 1	N E	29·86	29·80	48	36	—	02	98	
2	N W	29·93	29·81	49	32	—		90	☉
3	N W	30·04	29·93	46	33	—		71	
4	W	29·93	29·84	54	43	—		68	
5	S W	29·84	29·64	55	44	—	08	66	
6	S W	29·64	29·44	50	42	—		76	
7	W	29·65	29·45	51	29	39	05	67	
8	N W	29·92	29·65	44	25	—		74	
9	N W	29·92	29·38	46	30	—	11	70	☽
10	N W	29·65	29·38	51	42	—	11	100	
11	N E	30·01	29·65	42	37	—	04	86	
12	N E	30·00	29·94	47	41	—	—	80	
13	N E	29·94	29·89	46	40	—		74	
14	N	29·89	29·88	46	40	—		81	
15	W	29·88	29·65	47	39	—	54	96	
16	N W	29·69	29·65	42	36	—	52	100	
17	E	30·16	29·69	44	40	36	03	97	●
18	N E	30·16	30·07	45	30	—		70	
19	N E	30·07	29·84	38	32	—		80	
20	N W	29·84	29·18	43	32	—	15	69	
21	N W	29·62	29·18	43	28	—	—	74	
22	N W	29·85	29·62	40	26	—		77	
23	N W	29·96	29·85	37	21	—		72	
24	N W	30·07	29·96	40	26	—		81	☾
25	N W	30·07	29·81	34	23	—		91	
26	N W	29·97	29·81	41	31	—		100	
27	N W	29·97	29·82	39	24	—		99	
28	S W	29·97	29·74	49	31	—	17	100	
29	S W	29·75	29·74	53	44	—	08	100	
30	S W	29·81	29·75	54	47	40	22	100	
		30·16	29·18	55	21	1·15	2·12	69—100	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month.—1. Overcast. 2. *Cirrus, Cirrocumulus, Cirrostratus.* 3. Fine.
4. Cloudy: lunar halo. 5. Overcast: lunar halo. 6. *Cirrus, Cirrocumulus:* lunar
halo. 7. Cloudy: rain. 8. Fine: *Cirrus.* 9. Fine: a swallow seen this morn-
ing. 10. Rain. 11. Gloomy: drizzling. 12, 13. Cloudy. 14, 15. Gloomy.
16. Rainy. 17. Drizzling. 18, 19. Cloudy. 20. Fine day: rainy evening.
21. Overcast: windy. 22. Fine. 23, 24. Hoar frost in the mornings: misty.
25. Foggy day: lunar halo. 26. The roads and footpaths coated with ice:
cloudy: snow at night. 27. Misty: much rime on the trees: some snow early
this morning. 28. Hoar frost: misty: rain at night. 29. Very moist air: a
condensation on the *outside* of the windows: rain, with wind. 30. Rain.

RESULTS.

Winds: N, 1; NE, 6; E, 1; SW, 5; W, 3; NW, 14.

Barometer: Mean height
      For the month. .... 29·801 inches.
      For the lunar period, ending the 8th. .... 29·837
      For 15 days, ending the 13th (moon north) .... 29·781
      For 13 days, ending the 26th (moon south) .... 29·823

Thermometer: Mean height
      For the month.................................... 39·81°
      For the lunar period (as above) .... 44·44
      For 30 days, the sun in Scorpio.................... 40·61

Hygrometer: Mean for the month 83

Evaporation. .... 1·15 inch.

Rain .... 2·12

Rain at Tottenham...................... .... 2·00

      Stratford, Twelfth Month, 20, 1819.   L. HOWARD.

———————

Large Meteor. Tottenham, Eleventh Month, 18.—At about 10 minutes past five,
p. m. a brilliant meteor appeared in the west: it was first seen descending from
the zenith, at an angle of 45°, with a slow and steady motion towards the north.
It showed much larger than the planet Venus when at full, exhibiting a body of
yellow flame rather drawn out behind, and burning quietly without sparks.
When the combustion ceased, there remained a matter faintly luminous, which
gradually became extinct as it passed below the westernmost stars of Ursa Major.
The twilight was pretty strong in the south-west at the time.
    Præternatural Vegetation.—Some of the horse chesnut trees on our green (at
Tottenham) have this year exhibited a pretty complete double vegetation. The
trees in question are rather unhealthy, and probably stand in a bed of dry gravelly
soil; the others in loam. Towards the decline of the summer, after having flowered
abundantly and perfected the fruit, they shed their leaves, which had been spoiled
by the drought and dust. Immediately on feeling the effect of the rains about the
autumnal equinox, they put forth leaves and blossoms a second time, exhibiting
for several weeks a very singular and pleasing contrast to the now mature and
brownish foliage of the more healthy individuals of the same species. The new
fruit set pretty well even on the branches on which the old remained, and was as
large as a pea; when the premature approach of winter, shortening the duration of
the autumnal season, notwithstanding the protracted summer had delayed its
arrival, brought down both new and old, with the foliage remaining on the respect-
ive trees, together.   L. H.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1820.

ARTICLE I.

On Arsenic. By Thomas Thomson, M.D. F.R.S.

IN a short article on arsenic inserted in vol. xiv. of the *Annals of Philosophy*, p. 466, I showed that the result of the most careful experiments hitherto made gave the composition of the two acids of arsenic as follows:

Arsenious acid .... 4·75 arsenic + 1·5 oxygen
Arsenic acid . ... 4·75   + 2·5

or, if we consider an atom of arsenic to weigh 9·5, the composition of these acids may be stated as follows:

Arsenious acid .... 9·5 arsenic + 3 oxygen
Arsenic acid .... 9·5   + 5

According to this last notion, arsenious acid is a compound of 1 atom arsenic + 3 atoms oxygen, and arsenic acid of 1 atom arsenic + 5 atoms oxygen. The weight of an atom or integrant particle of arsenious acid will be 12·5, and of arsenic acid, 14·5. This mode of viewing the subject has the advantage of getting rid of the fractional parts of oxygen, which make their appearance when an atom of arsenic is reckoned to weigh 4·75, as I have done in the last edition of my System of Chemistry, and indeed, as has been hitherto done by every chemical writer whose works I have had an opportunity of seeing.

The best way of determining whether 14·5 or 7·25 be the number which represents the weight of an integral particle of arsenic acid seems to be a careful analysis of the arseniates. Unfortunately the greater number of these salts being insoluble

in water, we have not the means of obtaining them in regular
crystals, the only method in our power of preventing mechanical
mixtures from being substituted, for chemical compounds.
There are, however, two arseniates which can be procured in
regular crystals with great ease; namely, *arseniate of potash* and
arseniate of soda. The first of these has been long known to
chemists by the name of *Macquer's arsenical salt*. But I have
not hitherto met with an accurate account of the arseniate of
soda in chemical books. I shall, therefore, take the opportunity
of describing it here.

I. *Arseniate of Soda.*

To form this salt, I took a considerable quantity of arsenic
acid, which I had made by boiling nitromuriatic acid on arse-
nious acid till the whole was dissolved, and then distilling off the
nitromuriatic acid. Into this acid I dropped solution of carbon-
ate of soda till all effervescence was at an end, and till the liquid
ceased to redden litmus paper. The mixture was now evapor-
ated to the requisite consistency on a sand-bath, and set aside
for crystallization. But though the weather was peculiarly
favourable, not a single crystal could be obtained. The liquid
rendered cudbear paper violet, and gave other unequivalent
symptoms of containing an excess of alkali. Arsenic acid was,
therefore, dropped into it till all effervescence ceased. The
liquid, which now reddened litmus paper, being set aside,
yielded abundance of crystals of arseniate of soda, and by
repeated evaporations, I succeeded in extracting the whole of
the salt in tolerably regular crystals to the very last drop. All
the crystals thus obtained possessed the very same properties ;
therefore, though the liquid reddened litmus paper, it contained
no uncombined arsenic acid.

By dissolving the arseniate of soda thus formed in hot water,
and setting it aside in a cool place, I obtained it in large regular
transparent crystals, consisting of rhomboidal prisms, the faces of
which were inclined to each other at angles of 64° and 116°.
The bases of these prisms were rhombs, which, as far as I could
make out, had likewise angles of 64° and 116°.

These crystals remained unchanged though exposed to the
open air for a week in my laboratory ; but when I carried them
into my library, they speedily effloresced on the surface, and
became white and opaque ; but did not lose their form or
fall to powder after a month's exposure.

The taste is cooling, and bears some resemblance to carbonate
of soda, but is not so strong. It is very singular that though
the liquid from which these crystals were deposited reddened
litmus paper, the crystals themselves render cudbear paper
strongly *purple*, and even perceptibly affect litmus paper rendered
red by acetic acid. Now these effects are usually produced by
alkaline bodies.   3

The specific gravity of these crystals is 1·759, that of water being reckoned 1·000. Now if the salt be a compound of 1 atom arsenic acid, 1 atom soda, and 20 atoms water, as we shall immediately find reason to conclude its constituents to be, the mean specific gravity of such a compound (supposing no condensation) would be 1·078. Hence it appears that the 22 volumes of which it consists are condensed into about two-thirds of their original bulk.

One hundred grains of water at the temperature of 45° dissolve 10·132 gr. of this salt after being deprived of its water of crystallization. Now this is equivalent to 22·268 gr. of the crystallized salt. The specific gravity of this liquid (containing very nearly $\frac{1}{11}$th of its weight of the dry salt) at the temperature of 60° is 1·0903. Now the specific gravity of such a solution, supposing no condensation to have taken place, would have been 1·0698. Thus there is a condensation which scarcely exceeds two per cent. The salt is much more soluble in water of the temperature of 60°, and when the temperature amounts to 120°, the liquid dissolves more than its own weight of the crystallized salt.

This salt does not dissolve in alcohol; but when a crystal of it is suspended in that liquid, its surface is deprived of its water of crystallization, which renders it opaque and white.

When heated, it speedily undergoes the watery fusion, the water of crystallization being more than sufficient to keep it in solution at a boiling temperature. When kept for some time in a temperature of between 500° and 600°, it loses the whole of its water of crystallization, and is converted into a white powder. When this powder is exposed to a red heat, it undergoes the igneous fusion, and becomes liquid and transparent like water. By this treatment it sustains an additional loss of weight; but this loss is partly at the expense of the acid of the salt, which seems to undergo a partial decomposition; for the salt after this treatment is no longer completely soluble in water.

The greater number of the arseniates are insoluble in water. Hence the arseniate of soda occasions a precipitate when dropped into most of the earthy or metallic salts. The following table will put the reader in possession of the colour, solubility in nitric acid, &c. of the most remarkable of these precipitates.

Effect produced by dropping a saturated Solution of Arseniate of Soda at 45° into various saline Solutions.

1. *Muriate of Barytes.*—Becomes slowly milky, and a white precipitate falls. Redissolved by nitric acid.

2. *Muriate of Lime.*—A white precipitate. Redissolved by nitric acid.

3. *Nitrate of Strontian.*—A white precipitate. Redissolved by nitric acid.

4. *Muriate of Magnesia.*—No change at first; but on heating the mixture, a copious white precipitate fell.

5. *Muriate of Alumina.*⎫ White precipitates.   Redissolved
6. *Alum.*   ⎭   by nitric acid.

7. *Nitrate of Lead.*—A white precipitate. Redissolved by nitric acid.

8. *Sulphate of Nickel.*—An apple-green precipitate. Redissolved by nitric acid.

9. *Sulphate of Cobalt.*—A dirty-red precipitate. Redissolved by nitric acid.

10. *Nitrate of Silver.*—A flesh-red precipitate. Redissolved by nitric acid.

11. *Muriate of Tin.*—A white precipitate. Redissolved by nitric acid.

12. *Pernitrate of Mercury.*—A white precipitate. Redissolved by nitric acid.

13. *Protosulphate of Iron.*—A greenish-white precipitate. Redissolved by nitric acid.

14. *Sulphate of Copper.*—A bluish-green precipitate. Redissolved by nitric acid.

15. *Sulphate of Zinc.*—A white precipitate. Redissolved by nitric acid.

16. *Sulphate of Manganese.*—A white precipitate. Redissolved by sulphuric acid.

17. *Muriate of Iridium.*—No immediate change; but on heating the liquid, a brown precipitate fell.

18. *Sodamuriate of Rhodium.*—No immediate change; but on heating the liquid, a yellowish-white precipitate fell.

19. *Nitromuriate of Platinum.*—A light-brown precipitate. Redissolved by nitric acid.

20. *Nitromuriate of Gold.*—No immediate change; but on heating the liquid, a yellowish-white precipitate fell.

21. *Muriate of Antimony.*—A white precipitate. Redissolved by muriatic acid.

22. *Tartar Emetic.*—No change.

23. *Hydrosulphuret of Soda-and-Antimony.* No change.

It now only remains to determine the composition of this salt. I attempted in vain to determine in what proportion the acid and base unite by saturating the acid with the alkali, or *vice versâ*, noting the quantities of each employed. I could not determine when I had reached the requisite point of saturation, as the liquid still continues to act upon vegetable colours when that point has been reached. Fortunately, the analytical method in the present case is attended with little difficulty.

Fifty grains of the crystals, by cautious exposure to a heat slowly raised to 550°, lost 27·25 gr. of their weight, which I consider as the water of crystallization of the salt. When the same weight of salt is exposed to a red heat, it sustains a loss of

weight amounting to 29 gr.; but this cannot be allowing to the loss of water, for the residual salt is no longer completely soluble in water. The most accurate experiments which I have been able to make give 28·31 of water in 50 of salt, or 56·62 in 100 of salt.

Fifty grains of the salt were dissolved in water, and precipitated by nitrate of lead. The precipitate being washed, dried, and heated to redness, weighed 49·8 gr. Now from the analysis of arseniate of lead by Berzelius compared with my own experiments upon the same salt, I consider it as established that it is a compound of

<div style="text-align:center">

Arsenic acid . . . .   7·25
Protoxide of lead . . . .   14·00

</div>

According to this statement, 49·8 gr. of arseniate of lead contain 17 gr. of arsenic acid; therefore, arseniate of soda must be a compound of

Arsenic acid.	17·00	or 34·00
Soda	4·69	9·38
Water	28·31	56·62
	50·00	100·00

Now the weight of an integrant particle of soda is 4, and we have 4·69 : 17 :: 4 : 14·5 ; so that the weight of acid in this salt bears to that of the base the proportion of 14·5 to 4. Therefore, if we consider this salt as a compound of an atom of acid and an atom of soda, the weight of an atom of arsenic acid ·will be 14·5.

II. *Arseniate of Potash.*

This salt has been long known, but has never been accurately described. It was first formed by Macquer, who obtained it by distilling in a retort a mixture of equal parts of nitre and arsenious acid.* This salt usually crystallizes in four-sided rectangular prisms terminated by very short four-sided pyramids. I have some very fine crystals of it obtained by spontaneous crystallization. They are nearly three inches long, and of a proportional thickness. In them the prisms gradually diminish in size till they terminate in a point; so that the crystals have the appearance of consisting of enormously long octahedrons. This salt has a saline and cooling taste, somewhat similar to that of nitre. It is not in the least altered by exposure to the air. Its specific gravity is 2·638, and it has a much firmer and more solid texture than the crystals of arseniate of soda, indicating obviously from its appearance that it contains much less water of crystallization. It may be kept in a heat of 550° for a considerable time without melting or undergoing any remarkable

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* Mem. Par. 1764, p. 223.

change. The portions of the salt next the vessel, however, become whiter a little, and probably lose part of their water of crystallization, though the salt loses no sensible weight. In a red heat, it melts, and becomes as liquid as water. In that state it is nearly colourless, having only a slight tinge of yellow, and sometimes of green, both of which shades disappear when the salt becomes cool. On concreting, it cracks in all directions, indicating that it occupies a smaller bulk when solid than when liquid. The congealed salt is opaque, or only translucent, and white: 100 gr. of the salt by this treatment lose 7·5 gr. of their weight. The salt is completely soluble in water; therefore, the 7·5 gr. may be reckoned the water of crystallization.

Arseniate of potash is insoluble in alcohol: 100 parts of water at the temperature of 42° dissolve 19·047 gr. of the salt. The specific gravity of this solution (at 60°) is 1·1134. It is much more soluble in hot than in cold water. Hence a saturated solution in hot water crystallizes very readily on cooling.

The constituents of this salt, according to my analysis of it, are as follows:

$$
\begin{array}{lr}
\text{Arsenic acid.} \dots\dots\dots\dots\dots & 65\text{·}426 \\
\text{Potash.} \dots\dots\dots\dots\dots\dots & 27\text{·}074 \\
\text{Water} \dots\dots\dots\dots\dots\dots & 7\text{·}500 \\
\hline
 & 100\text{·}000
\end{array}
$$

Now the weight of an integrant particle of potash is 6, and 27·074 : 65·426 :: 6 : 14·5 very nearly. As far, therefore, as this salt is concerned, the weight of an atom of arsenic acid may be 14·5.

If you mix together a solution of arsenic acid and soda in the proportion of 7·25 arsenic acid + 4 soda, the liquid acts like an alkali upon vegetable blues, and cannot be made to yield crystals. We have, therefore, no accurate means of determining whether the acid and alkali form a salt in these proportions or not. The same observation applies to a mixture of 7·25 arsenic acid with six parts of potash.

III. *Arseniate of Copper.*

No fewer than five varieties of this salt all crystallized, but varying in the colour and shape of the crystals, have been described by Count Bournon, and analyzed by Chenevix. Unfortunately the analysis was made at a time (1801) when the necessity of minute accuracy was not so much felt as at present. Hence we are not to expect exact agreement between the atomic theory and the analytical results of Mr. Chenevix. Four of these five varieties occur crystallized in the copper mine of Huel Garland, in Cornwall. The fifth variety was formed artificially by Mr. Chenevix. He poured arseniate of ammonia into nitrate of copper, and filtered the liquid to get rid of a green-coloured

precipitate which fell. The liquid was now concentrated by evaporation, and a quantity of alcohol poured into it. A precipitate fell consisting of rhomboidal crystals of a blue colour. Chenevix found the constituents of this salt as follows :

$$Acid \dots \dots \dots \dots \dots \dots 14 \cdot 50$$
$$Black\ oxide\ of\ copper\ . \ \dots \ 12 \cdot 83$$
$$Water. \dots \dots \dots \dots \dots \ 8 \cdot 82$$

Now the number which represents the weight of an integrant particle of black oxide of copper is 10. I think, therefore, that it is not unlikely (if we suppose an atom of arsenic acid to weigh 14·5) that this salt is a compound of 1 atom of arsenic acid = 14·5 + 1 atom of black oxide of copper = 10 + 8 atoms of water = 9; so that this arseniate rather favours the opinion that an atom of arsenic acid weighs 14·5.

The second of Mr. Chenevix's varieties of arseniate of copper occurs in thin hexahedral plates, has a fine emerald green colour, and a specific gravity of 2·548. Its constituents were found to be :

$$Arsenic\ acid \dots \dots \dots \dots 14 \cdot 50$$
$$Black\ oxide\ of\ copper \dots \dots 13 \cdot 50$$
$$Water. \dots \dots \dots \dots \dots \ 6 \cdot 07$$

This (reckoning the weight of the atoms as before) is a compound of 1 atom acid + $1\frac{1}{3}$ oxide + 6 atoms water, making

$$Acid. \ . \dots \dots \dots \dots \dots 14 \cdot 50$$
$$Oxide. \dots \dots \dots \dots \dots 13 \cdot 33$$
$$Water. \dots \dots \ \dots \dots \dots \ 6 \cdot 75$$

The third and fourth varieties of Chenevix obviously agree in their composition; or if there be any difference, it consists in the water. They agree in colour (green) and in specific gravity, viz. 4·280. How far the crystalline forms can be reconciled remains still to be determined. The constituents of these two varieties, according to the analysis of Chenevix, are as follows;

	First Variety.	Second Variety.
Acid	14·5	14·5
Oxide.	25·0	26·1
Water	10·5	7·4

They obviously consist each of 1 atom of acid united to $2\frac{1}{2}$ atoms of oxide; while the first contains 9 atoms of water = 10·125, and the second, 7 atoms water = 7·805.

Mr. Chenevix's first variety occurs crystallized in obtuse octahedrons. Its colour varies from blue to green, and its specific gravity is 2·881. Its constituents, according to Mr. Chenevix's analysis, are as follows :

$$Acid. \dots \dots \dots \dots \dots \dots 14 \cdot 5$$
$$Oxide \dots \dots \dots \dots \dots \dots 50 \cdot 0$$
$$Water \dots \dots \dots \dots \dots \dots 35 \cdot 7$$

This is obviously equivalent to 1 atom of acid = 14·5 + 5 atoms oxide = 50 + 32 atoms water = 36.

We see from the preceding statement that the composition of these salts is not inconsistent with the notion that the weight of an atom of arsenic acid is 14·5; but a new analysis of them would be desirable. Want of the requisite specimens prevents me from having it in my power to undertake it.

IV. *Arseniate of Iron.*

There occur in the copper mines of Cornwall small cubic crystals of a dark-green colour, which Mr. Chenevix recognised to be an arseniate of iron usually contaminated with a portion of copper. This salt (abstracting the impurities) he found composed as follows:

Arsenic acid.. .... 14·50
Protoxide of iron.......... 20·91
Water. .... 4·83

Now an integrant particle of protoxide of iron weighs 4·5. I conceive, therefore, that the salt in question is a compound of 1 atom arsenic acid = 14·5 + 5 atoms of protoxide of iron = 22·5 + 4 atoms water = 4·5. This salt then is not inconsistent with the notion that an atom of arsenic acid weighs 14·5.

V. *Arseniate of Lead.*

Beautiful specimens of this salt occur in Huel Unity mine, near Redruth, in Cornwall. It has a wax-yellow colour, and is crystallized in large six-sided prisms. Mr. Gregor, who analyzed these crystals, found their constituents (abstracting a small quantity of muriate of lead) as follows:

Arsenic acid. .... 14·5
Protoxide of lead. .... 35·7

Now this is equivalent to 1 atom acid = 14·5 + 2¼ atoms protoxide of lead = 35 (for an atom of protoxide of lead weighs 14). The composition of this salt then is not inconsistent with the notion that an atom of arsenic acid weighs 14·5.

There is an arseniate of lead which precipitates in a white powder, when arseniate of soda is dropped into nitrate of lead. This powder is a compound of

Arsenic acid . ... 7·25
Protoxide of lead.......... 14·00

But it is not inconsistent with the notion that an atom of arsenic acid weighs 14·5. We have only to consider the salt as a subbinarseniate or a compound of 1 atom acid + 2 atoms oxide of lead.

Thus I have gone over all the crystallized arseniates with which I am acquainted, except the arseniate of ammonia, without

finding the composition of any of them inconsistent with the notion that an atom of arsenic weighs 14·5. I have not yet satisfied myself sufficiently respecting the constitution of the two varieties of arseniate of ammonia which are known to exist. But from the experiments which I have made, I may venture to say, that they will not be found to militate against the number 14·5 as the equivalent for arsenic acid. Upon the whole then the present state of our knowledge seems to make the choice of that number preferable to 7·25, its half, which I adopted in the fifth edition of my System of Chemistry. It has the advantage of destroying the fractional parts of oxygen, which, on the supposition that 7·25 is the weight of an atom of arsenic, enter into the composition of arsenious and arsenic acid. We may, therefore, fix upon the following numbers as representing the weight and composition of arsenic, arsenious and arsenic acid :

Arsenic, weight of its atom, 9·5.
Arsenious acid composed of 1 atom arsenic + 3 atoms oxygen = 12·5 weight.
Arsenic acid composed of 1 atom arsenic + 5 atoms oxygen = 14·5 weight.

ARTICLE II.

Experiments to determine the Composition of different inorganic Bodies which serve as a Basis to the Calculations relative to the Theory of Chemical Proportions. By J. Berzelius.

In a memoir published several years ago, I stated some experiments by which I endeavoured to obtain results sufficiently exact to serve for data, by means of which the composition of various compounds might be calculated with more certainty than they could be determined by analysis itself. As in these experiments I employed methods, such that the results depended as little as possible on the dexterity of the experimenter, I entertained some hopes of attaining my object; but I met with so many difficulties inseparable from that enterprize that none of my results could be considered as normal.

After six years of continual experiments on this subject, and after having acquired much experience and having discovered several improvements in the analytical methods, I resolved upon resuming these researches, which I consider as of the greatest importance. My object has been not to obtain results which are absolutely exact, which I consider as only to be obtained by accident, but to approach as near accuracy as chemical analysis

* Translated from Afhandlingar i Fysik, &c. v. 379.

can go, and to give an equal degree of precision to the analyses
of the most important bodies. If by these efforts the inevitable
errors in the results become proportional, even though they are
not absolutely correct, they will be of the same utility to us as if
they were so, because the mistakes are all within the limit of our
observation, and as nearly as possible proportional for all combi-
nations.

In endeavouring to determine to what degree of accuracy it
was possible to arrive, I have found that by very simple methods
it was possible to come within one-thousandth part of the weight
employed; so that variations in the results of experiments made
by the same process differ only from each other in the ten-thou-
sandth parts; but even this degree of accuracy requires much
care and attention in all the circumstances which may contribute
to render the results inaccurate, and which usually vary with the
manner of operating. I have never been able, except accident-
ally, to make the results coincide beyond the ten-thousandth
part, and indeed very frequently, notwithstanding all my care,
they have not agreed perfectly beyond the hundredth part.

Some of the experiments, which I intend to relate, were made
with the views just stated. Others are of an older date, and
have been already related in the third volume of the *Annals of
Philosophy* by Dr. Thomson. But among these last several have
been repeated, and some mistakes in my original memoir have
been corrected. I shall first give a description of the experi-
ments in which I endeavoured to obtain the greatest possible
precision; I shall then state those whose accuracy I cannot gua-
rantee to the same degree.

*Experiments to determine the exact Composition of the Bodies,
 which in a great Number of Analyses serve as Bases to the
 Calculations of Chemical Proportions.*

Muriate of Potash and Muriate of Silver.—Of all the methods
to determine the exact quantity in potash and in the oxide of
silver, none seems to me more likely to be exact than to deter-
mine in the first place the quantity of oxygen in the oxymuriate
of potash, and afterwards to analyze the common muriate. By
experiments already known, it has been established that oxymu-
riate of potash exposed to a red heat loses ten times the quantity
of oxygen contained in the potash of the residual muriate. If we
determine accurately the quantity of oxygen disengaged from the
oxymuriate, and then calculate, by the way just stated, the quan-
tity of oxygen which the potash should retain, the error resulting
will amount only to $\frac{1}{5}$th of the error of observation in the analysis
of the oxymuriate.

It is long since I published an analysis of the oxymuriate of
potash by which I had found that 100 parts of that salt give out
38·845 parts of oxygen; but to reach a greater degree of preci-
sion I considered it as requisite to repeat the experiment. In

my new researches, a difficulty presented itself which I was not aware of in my former ones, and which laid me under the necessity of making in the first place a great number of experiments in order to find a method of operating which should always give the same results. The difficulty consists in this, that just when the oxymuriate begins to be decomposed, the oxygen carries along with it a portion of oxymuriate in the form of a white smoke, and deposits it in the tubes. This smoke is deposited so slowly that when I passed the oxygen through a glass tube two feet in length, curved in different directions, and drawn out at the end so as to form a tube almost capillary, the last half of the tube was not covered with dust; but a piece of glass against which I allowed the jet of gas to strike became entirely covered with it at the place where the gas struck it. As the formation of this saline powder seemed to be merely the mechanical effect of the boiling (for it increased with this last), I endeavoured to avoid boiling by mixing it with from six to ten times its weight of fused muriate of potash. By this method I gained my object, and I then proceeded in the following manner: Oxymuriate of potash, the solution of which was not at all rendered muddy by nitrate of silver, was reduced to a fine powder, and dried in a temperature higher than 212°, but not sufficiently so to soften the salt. It was then put while hot into a retort which contained muriate of potash in powder. It had been heated to disengage all moisture, and its weight was known. When the mixture had recovered the temperature of the atmosphere, the retort was weighed again. It was then shaken to mix the two salts. By means of a caoutchouc tube, a glass tube 16 inches long, and a line in diameter, was attached to the retort. Into this tube some pieces of dry muriate of lime had been put. It was curved into a spiral, and the opening surrounded by filtering paper to retain all the saline smoke in powder. Care was taken to weigh the tube both alone and in connexion with the retort. The quantity of oxymuriate decomposed varied from 15 to 10 grammes. The retort was heated in a sand-bath till it became red within, and till the muriate began to soften. When the experiment was finished, the oxygen gas within the retort was changed for atmospheric air. The retort was then weighed along with the tube, and finally the tube alone.

In all these experiments, a trace of sublimate was found in the beak of the retort. It was oxymuriate undecomposed. It weighed in all 0·003 gr. The tube with the pieces of muriate of lime and paper had increased in weight from 0·02 to 0·023 gr. By subtracting this quantity of weight from what the retort had lost, we obtained the quantity of oxygen disengaged, and at the same time that of the muriate of potash remaining in the retort. Four experiments were made in this manner, and taking into the account the small quantity of sublimate, 100 parts of the oxymuriate gave

In the first .... 39·146 of oxygen gas
second. .... 39·150
third. .... 39·150
fourth .... 39·149

These experiments vary only in the ten-thousandth parts, and two of them even agree in them. We may, therefore, consider it as exceedingly near the truth that 100 parts of oxymuriate of potash, when pure and dry, give out 39·15 parts of oxygen gas.

To be able to make use of this result, it is necessary to know the exact composition of muriate of potash. I took, therefore, muriate obtained by these experiments, in which the best reagents could not detect the least excess of alkali, and I decomposed it by a solution of crystallized nitrate of silver : 10 grammes of muriate of potash gave in two experiments 19·24 grammes of fused muriate of silver; but it was necessary to know with the same degree of precision how much muriate of silver is obtained from a given weight of silver. Different chemists have made experiments on the subject, but the results do not always agree with each other. Those who have come nearest the truth I consider as the following. Wenzel found that 100 silver gave 131·4 of muriate of silver; Davy, 132·5; Bucholz, Rose, Marcet, and Gay-Lussac, 133·3 ; and finally, in my former experiments already described, I found from 132·7 to 132·75 ; for the degree of accuracy requisite in experiments which are to serve as a basis to our calculations, the difference between 132·5 and 133·3 is too much. Even the difference between 132·7 and 132·75 has a very sensible influence on the results deduced from it. It was, therefore, necessary to examine if the results of my old experiments were exact ; and in that case which of the two was nearer the truth.

1. I dissolved silver purified with the requisite care in pure nitric acid in an inclined phial, and the liquid was evaporated to dryness in the phial to get rid of all excess of acid. The nitrate of silver was then dissolved in water, and the clear solution was poured into a solution of sal-ammoniac. The precipitate was collected on a filter, well washed, dried, and then fused upon a watch glass of a known weight. A current of sulphuretted hydrogen gas was then passed through the filtered liquid, but no trace of silver could be discovered. Twenty grammes of silver gave in this way 26·54 grammes of muriate of silver; that is to say, 100 of metal gave 132·7 of muriate. This experiment can only err by a loss, since any excess of weight is impossible.

2. To compare this result with that of an experiment in which no loss could be sustained, and in which consequently the error must be in excess, I dissolved pure silver in an inclined phial, evaporated the solution to dryness, redissolved the nitrate in water, and added to it in the same phial pure muriatic

2

acid* as long as any precipitate fell. I immediately evaporated it to dryness, and as towards the end the two acids destroy each other, so that a little nitrate of silver might be again formed, I poured water on the dry mass, added muriatic acid, and evaporated the whole to dryness a second time. The phial with its contents was then exposed to a spirit of wine lamp till the muriate of silver was entirely melted: 20 grammes of silver treated in this manner gave in two experiments 26·556 and 26·558 grammes of fused muriate of silver, equivalent to 132·78 and 132·79 of muriate from 100 of silver.

Though the acids employed in these experiments were as pure as they can be obtained by the best known methods of preparing them, they always left visible traces, when evaporated, on a watch-glass. But, as in these experiments, it had been necessary to add these acids in considerable quantities, it is obvious that the small quantity of foreign matter which they contained must have acted on the balance, and rendered the weight a little greater than it would have otherwise been. As the true point must lie between 132·7 and 132·79, I think we may adopt 132·75 as sufficiently near the truth, observing always that the uncertainty in the results exists only in the ten-thousandth parts, which constitute the ordinary limit beyond which I have not been able to carry analytical experiments.

Admitting that 100 of oxymuriate of potash give 39·15 of oxygen ; that 100 of muriate of potash give 192·4 of muriate of silver ; and that 100 of silver give 132·75 of muriate of silver ; we may conclude from these three data by a calculation too simple to render it necessary to give an account of it here, the composition of the following bodies:

Muriate of potash is composed of

| Muriatic acid | 36·743 | 100·000 |
| Potash | 63·257 | 174·882 |

Potash is composed of

| Potassium | 83·0484 | 100·000 |
| Oxygen | 16·9516 | 20·412 |

Oxide of silver is composed of

| Silver | 93·112 | 100·0000 |
| Oxygen | 6·888 | 7·3986 |

Muriate of silver is composed of

| Muriatic acid | 19·0966 | 100·000 |
| Oxide of silver | 80·9034 | 423·653 |

The *capacity of saturation of muriatic acid,* that is to say, the

* This acid had been prepared by saturating water with muriatic acid gas, which had passed through an intermediate vessel to deposit all impurity.

quantity of oxygen which must exist in the base capable of satu-
rating 100 of the acid, is then 29·184, instead of 29·454 as I had
found it in my former experiments. It is equally evident that
these data are sufficient for calculating the composition of
muriatic acid, oxymuriatic acid, and oxymuriate of potash. But
I pass by these calculations in silence as they are foreign to the
object which I have in view.

Oxide of Lead.

It is of very great importance to know accurately the compo-
sition of the oxide of lead, especially in consequence of its great
influence in the analysis of organic bodies. Although in my
former and numerous experiments on this oxide, I obtained
results which agreed with each other, I thought it necessary to
repeat them, in order, if possible, to bring them to a greater
degree of precision.

1. *Composition of Oxide of Lead found by direct Analysis.*—
Among the analytical methods which depend the least upon the
dexterity of the operator, I conceive that the following occupies
the first rank. In a glass globe blown in the middle of a piece
of barometer tube, a quantity of oxide of lead was introduced
which had just been exposed to a red heat. The glass ball was
now heated by a spirit lamp, and a current of hydrogen gas pre-
pared from distilled zinc, and sulphuric or muriatic acid was
passed through it. The oxide becomes at first black, small
globules of reduced lead are seen, and after an interval of two
hours, it is converted into melted lead. The lamp is withdrawn,
and the lead is allowed to cool, while the hydrogen gas still con-
tinues to pass. If we weigh in the first place the tube of glass
alone, then when it contains the oxide of lead, and lastly when
it contains the reduced lead, the only possible error must proceed
from inaccuracy in weighing, provided we have employed a
hydrogen gas absolutely free from sulphur.

I made in this way three experiments:

(1.) 21·9425 grammes of oxide of lead left 20·3695 grammes
of metallic lead; that is to say, that 100 lead had been united
with 7·7223 oxygen.

(2.) 10·8645 gr. of oxide of lead yielded 10·084 gr. of lead.
Equivalent to 100 lead united to 7·74 oxygen.

(3.) 11·159 gr. of oxide of lead yielded 10·359 gr. of lead.
Equivalent to 100 lead united to 7·7228 oxygen.

2. *Composition of Oxide of Lead determined by a Calculation
which has for its Basis the Analysis of Nitrate of Lead.*—From
an analytical experiment on nitrate of lead, 100 of the nitrate
yield 67·31 of oxide of lead; but we know that nitric acid is
composed of 1 volume of azotic gas and $2\frac{1}{2}$ volumes of oxygen
gas, and that (neglecting the oxygen probably contained in the
azote) this acid contains five times as much oxygen as the base
by which it is neutralized. If we determine the composition of

the acid by weight from the specific gravity of the gases, and then calculate from the preceding data the composition of oxide of lead, we find that 100 lead must unite to 7·7448 oxygen.

3. *The same calculated from the Muriate of Lead.*—Fused muriate of lead was pounded, weighed, and dissolved in boiling water. It left a small portion of submuriate undissolved, which was separated, dried, weighed, and subtracted from the weight of the muriate employed. This precaution is indispensable. I have never been able to dissolve fused muriate of lead without its leaving an insoluble residue of submuriate. I have even fused it in a retort filled with muriatic acid gas without being able to prevent a partial decomposition: 100 of muriate of lead precipitated by nitrate of silver produced 103·35 of muriate of silver. Muriate of lead, therefore, is composed of

Muriatic acid ...... 19·74 ...... 100·000
Oxide of lead ...... 80·26 ...... 406·585

It follows that 100 lead combine with 7·7316 oxygen.

4. *The same calculated from the Carbonate of Lead.*—I prepared the carbonate employed in these experiments by precipitating a solution of nitrate of lead by means of carbonate of soda obtained by the calcination of tartrate of soda, or by carbonate of ammonia, both added in excess, and by washing the precipitate with pure water. The carbonate of lead was strongly dried in a heat some degrees above 212°. It was decomposed in a glass retort exactly weighed, and the gas was passed through a tube filled with muriate of lime, and exactly weighed. The carbonic acid gas disengaged in two experiments had a stronger smell than usual resembling *oleum cornucervi*, when the carbonate of lead had been precipitated by carbonate of ammonia. This circumstance induced me to sublime a portion of sal-ammoniac a second time to prepare carbonate of ammonia by distilling it with carbonate of potash; but this precaution was unavailing. I had the same odour and the same result in weight; and we see from what follows, that the substance which communicated the odour had no appreciable weight. The analysis gave:

	Precipitated by carbonate of soda.	Precipitated by carbonate of ammonia.
Carbonic acid	16·442	16·447
Oxide of lead	83·333	83·333
Moisture stopped by the muriate of lime.	0·225	0·220

The results of these experiments vary only in the sixth figure, and may, therefore, be considered as very near the truth. It follows from them that 100 of carbonic acid are neutralized by 506·823 of oxide of lead. According to the specific gravities of oxygen and carbonic acid gases, as determined by Arago and Biot, carbonic acid contains 72·623 per cent. of oxygen. We

know that the base which saturates that acid contains half of
that quantity of oxygen. The following is a comparison of the
quantity of oxygen which we have found combined with 100 of
lead :

> 7·7218 carbonate of lead.
> 7·7223 reduction by hydrogen.
> 7·7228 ditto.
> 7·7316 muriate of lead.
> 7·7400 reduction by hydrogen.
> 7·7448 nitrate of lead.

We see then that 100 lead combine with more than 7·72 oxy-
gen, and with less than 7·75. Three of these numbers differ only
in the sixth figure, and two only in the seventh. All circum-
stances considered, I conceive I may admit as the medium of
the determinations which are entitled to the greatest confidence
7·725, as the true quantity of oxygen which can unite with 100
lead. On that supposition, oxide of lead is composed of

> Lead .... 92·829 ...... 100·000
> Oxygen .... 7·171 ...... 7·725

Sulphuric Acid.

It is known that in the neutral sulphates, the sulphuric acid
contains three times as much oxygen as the base by which it is
neutralized. We have just determined the composition of oxide
of lead. From this it is easy to determine the composition of
sulphuric acid by observing how much lead is yielded by a given
weight of sulphate of lead. I dissolved lead in pure nitric acid
in a Florence flask, inclined in such a manner that the efferves-
cence could drive nothing out of the vessel. The liquid was
then poured into a platinum crucible of a known weight. Pure
dilute sulphuric acid was mixed with it. It was evaporated to
dryness, and the excess of sulphuric acid was driven off by
exposing it to a red heat. This experiment appears very simple,
but it is of very difficult execution, in consequence of the weight
of the precipitate, which occasions portions of it to be driven out
of the vessel if the temperature be raised a little above 212°. 1
made four experiments, and employed 10 grammes of lead each
time. They furnished the following results :

> First . ... 14·6380 of sulphate of lead.
> Second. ...... 14·6400
> Third. .... 14·6440
> Fourth .... 14·6458

Among these experiments, the first differs in the fourth figure;
but the others differ only in the sixth. I conceive that I may
choose the result of the third experiment ; that is to say, 14·644,
as nearest the truth. In this quantity of sulphate there is
10·7725 of oxide of lead and 3·8715 of sulphuric acid, the

oxygen of which ought to be 0·7725 + 3 × 2·3175, and the
sulphur, 1·5540.   Of consequence, sulphuric acid is composed of

$$\text{Sulphur} \ldots \ldots \ldots 40·1395 \ldots \ldots 100·00$$
$$\text{Oxygen} \ldots \ldots \ldots 59·8605 \ldots \ldots 148·44$$

Hence it follows that the capacity of saturation of sulphuric
acid ought to be $\frac{59·8605}{3} = 19·9535$.

It is known by old experiments that sulphur in order to pro-
duce sulphurous acid must combine with two-thirds of the
oxygen which exists in sulphuric acid.   Hence it is easy to cal-
culate the composition of sulphurous acid.   I thought, however,
that a verification of this calculation furnished by the determina-
tion of the specific gravity of sulphurous acid would not be
superfluous.   And though the experiments which I made with
this view have not given the result which I expected, I shall
here give an account of them.

If we suppose that oxygen gas when it unites with sulphur to
produce sulphurous acid preserves its volume just as when it
forms carbonic acid gas with carbon, it is evident that the differ-
ence between the specific gravity of sulphurous acid and oxygen
gas ought to indicate the quantity of sulphur contained in the
former gas.   I prepared sulphurous acid gas in the following
manner: I put copper into a retort, which I then filled com-
pletely with concentrated sulphuric acid. The beak of the retort
was then introduced into sulphuric acid, and the retort was
heated till the gas extricated ceased to drive out sulphuric acid.
The beak of the retort was then plunged under a glass jar filled
with mercury, and the disengagement of sulphurous acid gas
was continued.   The glass vessel had at its top a stop-cock of
brass, which could not be filled with mercury, and in which
there remained of consequence a little atmospherical air.   This
portion was removed by filling a sixth part of the vessel with
sulphurous acid gas, and then throwing out this mixture of gas
and air.   This operation was repeated five or six times before
the vessel was finally filled with sulphurous acid gas.   This gas
was then introduced till the gas within the glass was compressed
by a column of mercury, an inch in height, on the outside of the
vessel.   I then adapted to it, by means of a stop-cock furnished
with a screw, a thin glass matrass, which I had previously
exhausted of air.   On opening the stop-cock, the matrass was
filled with sulphurous acid gas, and care was taken to plunge the
glass in the mercury till the gas in the matrass was somewhat
compressed.   It was then carried to the place where it was to
be weighed.   It was left there an hour in order to acquire the
temperature of the place, which was 59°.   The stop-cock was
then opened, taking care not to touch the matrass with the hand,
in order not to alter the temperature.   The gas being thus
brought into equilibrium with the air, the stop-cock was shut.

and the matrass weighed. The barometer varied during the experiment between 24·6 and 24·7.

I continued these experiments for three days, making three each day. The weight of the sulphurous acid varied from 1·308 to 1·311, without it being possible to ascribe this variation to changes in the pressure of the atmosphere. The quantity of air which the air-pump extracted from the matrass weighed 0·576 to 0·578 gramme; but when the air was allowed to enter through a tube filled with muriate of lime, it always weighed 0·583 gramme. At each experiment, the matrass was weighed after being exhausted, to be sure that the air-pump had always extracted the same quantity of air. The stop-cock was examined each day by leaving the exhausted matrass for half an hour on the balance without its weight increasing by the entrance of air.

The errors of observation in this experiment ought always to have a tendency to give the specific gravity of the gas too small, in consequence of a mixture of air with the sulphurous acid gas. Only a single circumstance could contribute to increase the weight. The grease upon the stop-cock might have absorbed a small quantity of sulphurous acid. To determine this point, I weighed the stop-cock before and after each experiment; but its weight was not altered. It is possible likewise that a little sulphuric acid might evaporate, and remain suspended in the gas; but I always left the gas a long time standing on the mercury before putting it into the matrass; so that the vapour, supposing it to exist, had all the time necessary to be deposited.

If we admit 1·31 as the weight of the sulphurous acid gas in the preceding experiments, 58·3 : 131 :: 1·00 : 2·247. If in these 2·247 parts of sulphurous acid there is an equal volume of oxygen whose weight is 1·10359, 100 parts of sulphur are combined with 96·52 parts of oxygen instead of 98·954, as follows from the analysis of sulphuric acid. This deviation is too great to be merely an error of observation. Hitherto all circumstances speak in favour of the results drawn from the analysis of sulphuric acid, which agree so well with the experiments on the composition of the sulphurets, and which agree exactly with the analyses of other saline combinations, as we shall immediately see.

I have not been able to find the cause of this anomaly. Some chemists pretend that oxygen gas, when it unites with sulphur, diminishes in volume. This diminution has been supposed to go as far as $\frac{1}{12}$th. But whether it is owing to a small quantity of hydrogen in the sulphur, or constitutes an exception to what we consider as a general law, remains to be determined.

(To be continued.)

Article III.

Demonstration of Dr. Taylor's Theorem, with Examples.
By Mr. James Adams.

(To Dr. Thomson.)

SIR,   *Stonehouse, near Plymouth, Dec.* 22, 1819.

SHOULD you consider the following demonstration of *Dr. Taylor's theorem*, together with the accompanying examples, likely to benefit the young algebraist, your inserting them in the *Annals of Philosophy* will much oblige,

Your humble servant,

JAMES ADAMS.

Prop. 1.—If φ represent a function composed of known and unknown quantities, then will

$$\varphi_n = \varphi + \Delta \varphi + \frac{n(n-1)}{2} \Delta^2 \varphi + \frac{n(n-1)(n-2)}{2.3} \Delta^3 \varphi + \&c.$$

From the nature of increments

$$\varphi = \dots \dots \varphi$$
$$\varphi_1 = \dots \dots \varphi + \Delta \varphi = \varphi'$$
$$\varphi_2 = \varphi_1' = \varphi' + \Delta \varphi' = \varphi + \Delta \varphi + \Delta \varphi + \Delta^2 \varphi = \varphi + 2\Delta \varphi + \Delta^2 \varphi = \varphi''$$
$$\varphi_3 = \varphi_1'' = \varphi'' + \Delta \varphi'' = \varphi + 3\Delta \varphi + 3\Delta^2 \varphi + \Delta \varphi = \varphi'''$$
$$\varphi_4 = \varphi_1''' = \varphi''' + \Delta \varphi''' = \varphi + 4\Delta \varphi + 6\Delta^2 \varphi + 4\Delta^3 \varphi + \Delta^4 \varphi$$
$$\&c. \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots$$

It is evident that the coefficients of φ, $\Delta \varphi$, $\Delta^2 \varphi$, $\Delta^3 \varphi$, &c. are the same as the coefficients of the corresponding powers of a binomial; and, therefore, each in general is represented by $1, n, \frac{n(n-1)}{2}, \frac{n(n-1)(n-2)}{2.3}$, &c. Hence the value of φ_n, as stated in the proposition.

Prop. 2.—To find the *n*th increment of the function φ.

$$\Delta \varphi = \varphi_1 - \varphi$$
$$\Delta^2 \varphi = \varphi_2 - \varphi_1 - \varphi_1 + \varphi = \varphi_2 - 2\varphi_1 + \varphi$$
$$\Delta^3 \varphi = \varphi_3 - 2\varphi_2 + \varphi_1 - \varphi_2 + 2\varphi_1 - \varphi = \varphi_3 - 3\varphi_2 + 3\varphi_1 - \varphi$$
$$\dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \dots$$
$$\Delta^n \varphi = \varphi_n - n\varphi_{n-1} + \frac{n(n-1)}{2} \varphi_{n-2} + \frac{n(n-1)(n-2)}{2.3} \varphi_{n-3} + \&c.$$

Prop. 3.—If φ represent a function composed of known and unknown quantities, then will

$$\Delta \varphi = \frac{d\varphi}{1} + \frac{d^2\varphi}{2} + \frac{d^3\varphi}{2.3} + \frac{d^4\varphi}{2.3.4} + \&c.$$

By prop. 1, $\varphi_n = n\,d\varphi + \frac{n(n-1)}{2} d^2\varphi + \frac{n(n-1)(n-2)}{2.3}$ $d^3\varphi + \&c.$ Now since the increments $d\varphi$, $d^2\varphi$, $d^3\varphi$, &c. are always considered as " indefinitely or incomparably small," let

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them be expressed by the fractions $\frac{1}{\gamma'}, \frac{1}{\gamma''}, \frac{1}{\gamma'''}$. Then by substituting in the last equation, we shall have $\varphi_n = \varphi + n \Delta \varphi = \varphi$
$+ \frac{n}{\gamma'} + \frac{n(n-1)}{2 \cdot \gamma''} + \frac{n(n-1)(n-2)}{2 \cdot 3 \cdot \gamma'''} + $ &c.

By cancelling the function φ and dividing by n, we get
$$\Delta \varphi = \frac{1}{\gamma'} + \frac{n-1}{2 \cdot \gamma''} + \frac{(n-1)(n-2)}{2 \cdot 3 \cdot \gamma'''} + \frac{(n-1)(n-2)(n-3)}{2 \cdot 3 \cdot 4 \cdot \gamma''''} + $$ &c.

Now since the numerators and denominators of the fractions in the last equation increase together, the former being *finite whole numbers*, and the latter *indefinitely, or incomparably great whole numbers;* no sensible change will be made in the equation, by supposing each of the numerators equal to unity, according to which, and restoring the values of $\frac{1}{\gamma'}, \frac{1}{\gamma''}, \frac{1}{\gamma'''}$, &c. we get,

$$\Delta \varphi = d \varphi + \frac{d^2 \varphi}{2} + \frac{d^3 \varphi}{2 \cdot 3} + \frac{d^4 \varphi}{2 \cdot 3 \cdot 4} + $$ &c. which is commonly called Taylor's theorem.

This theorem is as universal in its application in the theory of increments, or differences, as the binomial theorem is, in the expansion of roots and powers.—(Mr. Barlow's Dictionary; Article, Increment.)

In order to find the increment of any function of a variable quantity, we must take the first, second, third, &c. differentials of the given function, and divide the results by 1, 2, 2 × 3, 2 × 3 × 4, &c. respectively, and we shall have the value of $\Delta \varphi$; which will always be finite unless the function be transcendental.

Example 1.—To find the increment of x.

The first differential of x is dx, and the second differential of x is zero, because dx is supposed constant; therefore $\Delta x = dx$.

Example 2.—To find the increment of x^2.

$dx^2 = 2 x dx$, and $d(2 x dx) \div 2 = dx^2 = \Delta x^2$ (example 1); therefore $\Delta (x^2) = 2 x \Delta x + \Delta x^2$ (dx constant).

Example 3.—To find the increment of x^3.

$d(x^3) = 3 x^2 dx$; $d(3 x^2 dx) \div 2 = 3 x dx^2$; $d(3 x dx^2) \div 6 = dx^3$; therefore, $\Delta(x^3) = 3 x^2 \Delta x + 3 x \Delta x^2 + \Delta x^3$ (dx constant).

Example 4.—To find the increment of x^m.

$d(x^m) = m x^{m-1} dx$, $d(m x^{m-1} dx) = m(m-1) x^{m-2} dx^2$, $d\left(m(m-1) x^{m-2} dx^2\right) = m(m-1)(m-2) x^{m-3} dx^3$, &c.

By writing Δx for dx, and substituting in the general theorem, we have $\Delta(x^m) = m x^{m-1} \Delta x + \frac{m(m-1)}{2} x^{m-2} \Delta x^2 + $

$\frac{m(m-1)(m-2)}{2 \cdot 3} x^{m-3} \Delta x^2 + $ &c. (dx constant.)

Example 5.—To find the increment of $x(x + \Delta x)$.

$d(x^2 + x dx) = 2 x dx + dx^2$, and $d(2 x dx) = 2 dx^2$, then

by substituting in the general theorem and writing Δx for $d x$, we get $\Delta \{x (x + \Delta x)\} = 2 \Delta x (x + \Delta x)$; ($d x$ constant.)

Example 6.—To find the increment of $x (x + \Delta x) (x + 2 \Delta x)$.

In order to simplify, put $w = \Delta x$, a constant quantity; then will $d \{x (x + \Delta x) (x + 2 \Delta x) = d (x^3 + 3 w x^2 + 2 w^2 x)$ $= 3 w x^2 + 6 w^2 x + 2 w^3$, $d (3 w x^2 + 6 w^2 x) = 6 w^2 x + 6 w^3$, and $d (6 w^2 x) = 6 w^3$. Then by substituting in the general theorem, we have the required increment; viz. $3 w x^2 + 9 w^2 x + 6 w^3 = 3 w (x + w) (x + 2 w)$, by restoring Δx, we get $\Delta \{x (x + \Delta x) (x + 2 \Delta x)\} = 3 \Delta x \{(x + \Delta x) (x + 2 \Delta x)\}$.

Example 7.—To find the increment of $x (x + \Delta x) (x + 2 \Delta x)$ $(x + 3 \Delta x)$.

Put $\Delta x = w$, a constant quantity; then will the above expression become $x^4 + 6 w x^3 + 11 w^2 x^2 + 6 w^3 x$, then will

$d (x^4 + 6 w x^3 + 11 w^2 x^2 + 6 w^3 x) = 4 w x^3 + 18 w^2 x^2 + 22 w^3 x + 6 w^4$

$d (4 w x^3 + 18 w^2 x^2 + 22 w^3 x) \div 2 = \ldots \ldots + 6 w^2 x + 18 w^3 x + 11 w^4$

$d (12 w^2 x^2 + 36 w^3 x) \div 6 \ldots = \ldots \ldots \ldots \ldots \ldots + 4 w^3 x + 6 w^4$

$d (24 w^3 x) \div 24 \ldots \ldots \ldots = \ldots \ldots \ldots \ldots \ldots \ldots + w^4$

The required increment $\ldots = 4 w x^3 + 24 w^2 x^2 + 44 w^3 x + 24 w^4 =$

$4 w (x^3 + 6 w x^2 + 11 w^2 x + 6 w^3) = 4 w \Big((x + w) (x + 2 w) (x + 3 w) \Big) =$

$4 \Delta x \Big((x + \Delta x) (x + 2 \Delta x) (x + 3 \Delta x) \Big)$. Hence we conclude

that $\Delta \Big(x (x + \Delta x) (x + 2 \Delta x) \ldots (x + n \Delta x) \Big) = (n + 1) \Delta x$

$\Big((x + \Delta x) (x + 2 \Delta x) \ldots (x + n \Delta x) \Big)$.

Example 8.—To find the increment of $\frac{1}{x}$.

Put $w = d x = \Delta x$, a constant quantity, then will

$d \left(\frac{1}{x} \right) = d (x^{-1}) = - w x^{-2} = - \frac{w}{x^2}$; $d (- w x^{-2}) \div 2 =$

$w^2 x^{-3} = \frac{w^2}{x^3}$; $d (2 w^2 x^{-3}) \div 6 = - w^3 x^{-4} = - \frac{w^3}{x^4}$; &c.

Therefore $\Delta \left(\frac{1}{x} \right) = - \frac{w}{x^2} + \frac{w^2}{x^3} - \frac{w^3}{x^4} + $ &c. $= \frac{1}{x + w} - \frac{1}{x} = -$

$\frac{w}{x (x + w)} = - \frac{\Delta x}{x (x + \Delta x)}$.

Example 9.—To find the increment of $\frac{1}{x^2}$.

Put $w = d x = \Delta x$, a constant quantity, then will

$d \left(\frac{1}{x^2} \right) = d (x^{-2}) = - 2 w x^{-3} = - \frac{2 w}{x^3}$; $d (- 2 w x^{-3}) \div 2$

$= 3 w^2 x^{-4} = \frac{3 w^2}{x^4}$, $d (6 w^2 x^{-4}) \div 6 = - 4 w^3 x^{-5} = -$

$\frac{4 w^3}{x^5}$; &c. Therefore $\Delta \left(\frac{1}{x^2} \right) = - \frac{2 w}{x^3} + \frac{3 w^2}{x^4} - \frac{4 w^3}{x^5} + $ &c. $=$

$\left(\frac{1}{x + w} \right)^2 - \frac{1}{x^2} = - \frac{2 w x + w^2}{x^2 (x + w)^2} = - \frac{2 x \Delta x + \Delta x^2}{x^2 (x + \Delta x)^2}$.

Example 10.—To find the increment of $\frac{1}{x^m}$.

Put $w = d\,x = \Delta\,x$, a constant quantity, then will

$$d\left(\frac{1}{x^m}\right) = d\,(x^{-m}) = -m\,w\,x^{-(m+1)} = -m\cdot\frac{w}{x^{m+1}},$$

$$d\,(-m\,w\,x^{-(m+1)}) \div 2 = \frac{m\,(m+1)}{2}\times\frac{w^2}{x^{m+2}},$$

$$d\left(m\,(m+1)\,w^2\,x^{-(m+2)}\right)\div 6 = \frac{m\,(m+1)\,(m+2)}{2.3}\times\frac{w^3}{x^{m+3}},$$

&c. . . . .

Therefore $\Delta\left(\frac{1}{x^m}\right) = -m\cdot\frac{w}{x^{m+1}}+\frac{m\,(m+1)}{2}\cdot\frac{w^2}{x^{m+2}}-$ &c. $=$

$$\frac{1}{(x+w)^m}-\frac{1}{x^m} = \frac{m\,w\,x^{m-1}+\frac{m\,(m-1)\,x^2}{2}x^{m-2}+\&c.}{x^m\,(x+w)^m}.$$

Example 11.—To find the increment of log. x.

Put $w = d\,x = \Delta\,x$, a constant quantity, and $y = \log. x$, then

$$d\,y = \frac{w}{x},\ \frac{d^2 y}{2} = -\frac{w^2}{2\,x^2},\ \frac{d^3 y}{6} = \frac{w^3}{3\,x^3},\ \frac{d^4 y}{24} = -\frac{w^4}{4\,x^4}+\&c.$$

therefore $d\,y+\frac{d^2 y}{2}+\frac{d^3 y}{6}+\frac{d^4 y}{24}+\&c. = \frac{w}{x}-\frac{w^2}{2\,x^2}+\frac{w^3}{3\,x^3}-\frac{w^4}{4\,x^4}$

$+$ &c.

or $\Delta\,(\log. x) = \frac{\Delta\,x}{x}-\frac{\Delta\,x^2}{2\,x^2}+\frac{6\,x^3}{3\,x^3}-\frac{\Delta\,x^4}{\Delta\,x^4}+$ &c.

Example 12.—To find the increment of x log. x.

Put $w = d\,x = \Delta\,x$, a constant quantity, and $y = x$ log. x, then

$$d\,y = w\,\log. x+w = w\,(\log. x+1),\ \frac{d^2 y}{2} = \frac{w^2}{2\,x},\ \frac{d^3 y}{6} = -$$

$\frac{w^3}{6\,x^2},\ \frac{d^4 y}{24} = \frac{w^4}{12\,x^3},\ \frac{d^5 y}{120} = -\frac{w^5}{20\,x^4},$ &c. Therefore, $d\,y+\frac{d^2 y}{2}+$

$\frac{d^3 y}{2.3}+\frac{d^4 y}{2.3.4}+$ &c. $= w\,(\log. x+1)+w$; or $\Delta\,(x\,\log. x)$

$$= \Delta\,x\,(\log. x+1)+\frac{\Delta\,x^2}{2.x}-\frac{\Delta\,x^3}{2.3..x^2}+\frac{\Delta\,x^4}{3.4..x^3}-\frac{\Delta\,x^5}{4.5.x^4}$$

$+$ &c.

Example 13.—To find the increment of $(\log. x)^m$.

Put $y = \log. x = l\,x$, then will $\Delta\,(\log. x)^m = \Delta\,(y^m)$; but by

example 4, $\Delta\,(y^m) = m\,y^{m-1}\,\Delta\,y+\frac{m\,(m-1)\,y^{-2}\,\Delta\,x^2}{2}+$ &c. by

substituting for y, we have $\Delta\,(\log. x)^m = m\,(l\,x)^{m-1}\,\Delta\,l\,x+$

$\frac{m\,(m-1)\,(l\,x)^{m-2}\,(\Delta\,l\,x)^2}{2}+$ &c. The values of $\Delta\,l\,x$, $(\Delta\,l\,x)^2$,

$(\Delta\,l\,x)^3$, &c. may be expanded into a series, if necessary, by

example 11.

Example 14.—To find the increment of a^x.

Suppose $d\,x = \Delta\,x$, a constant quantity, and $y = a^x$.

Then will log. $y = l\,y = x\,l\,a$, and $d\,(l\,y) = \frac{dy}{y} = d\,x\,l\,a.$

Therefore

$dy = y \cdot d\,x \cdot l\,a$

$d^2\,y = d\,y \cdot d\,x \cdot l\,a = y \cdot d\,x^2\,(l\,a)^2$

$d^3\,y = d\,y \cdot d\,x^2\,(l\,a)^2 = y \cdot d\,x^3\,(l\,a)^3$

$d^4\,y = d\,y \cdot d\,x^3\,(l\,a)^3 = y \cdot d\,x^4\,(l\,a)^4$

&c.................................

Then $\Delta\,y = \Delta\,(a^x) = d\,y + \frac{d^2\,y}{2} + \frac{d^3\,y}{2\cdot3} + \frac{d^4\,y}{2\cdot3\cdot4} + $ &c.

Or $\Delta\,(a^x) = y \cdot d\,x \cdot l\,a + \frac{y \cdot d\,x^2 \cdot (l\,a)^2}{2} + \frac{y \cdot d\,x^3\,(l\,a)^3}{2\cdot3} + $ &c,

$\qquad = a^x \left(\Delta\,x \cdot l\,a + \frac{\Delta\,x^2\,(l\,a)^2}{2} + \frac{\Delta\,x^3\,(l\,a)^3}{2\cdot3} + \text{&c.} \right).$

If $a = e$, be the number whose logarithm is unity, then $l\,a$, $(l\,a)^2$, $(l\,a)^3$, &c. are each equal to unity, we shall then have

$$\Delta\,(e^x) = e^x \left(\Delta\,x + \frac{\Delta\,x^2}{2} + \frac{\Delta\,x^3}{2\cdot3} + \frac{\Delta\,x^4}{2\cdot3\cdot4} + \text{&c.} \right) = e^x\,(e^{\Delta x} - 1).$$

Example 15.—To find the increment of sin. x (radius unity). Suppose $d\,x = \Delta\,x = w$, a constant quantity, and $y = \sin.\,x$.

Then $d\ y = d\quad(\sin.x)\qquad = +\,w\;\cos.x = +\,\Delta\,x\;\cos.x$

$\qquad d^2\,y = d\,(\quad w\;\cos.x) = -\,w^2\sin.\,x = -\,\Delta\,x^2\sin.x$

$\qquad d^3\,y = d\,(-\,w^2\;\sin.\,x) = -\,w^3\cos.\,x = -\,\Delta\,x^3\cos.\,x$

$\qquad d^4\,y = d\,(-\,w^3\cos.\,x) = +\,w^4\sin.\,x = +\,\Delta\,x^4\sin.x$

$\qquad d^5\,y = d\,(\quad w^4\sin.\,x) = +\,w^5\sin.\,x = +\,\Delta\,x_5\;\cos.x$

Therefore $\Delta\,y = \Delta\,(\sin.\,x) = d\,y + \frac{d^2\,y}{2} + \frac{d^3\,y}{2\cdot3} + \frac{d^4\,y}{2\cdot3\cdot4} + $ &c.

Or $\Delta\,(\sin.\,x) = \Delta\,x\cos.\,x - \frac{\Delta\,x^2}{2}\sin.\,x - \frac{\Delta\,x^3}{2\cdot3}\cos.\,x + \frac{\Delta\,x^4}{2\cdot3\cdot4}$

$\sin.\,x + \frac{\Delta\,x^5}{2\cdot3\cdot4\cdot5}\cos.\,x - $ &c.

Example 16.—To find the increment of cos. x (radius unity). Put $w = d\,x = \Delta\,x$, a constant quantity, and $y = \cos.\,x$.

Then $d\ y = d\,(\cos.\,x).\ \dots = -\ w\ \sin.\,x = -\ \Delta\,x\ \sin.\,x$

$\qquad d^2\,y = d\,(-\,w\ \sin.\,x) = -\,w^2\cos.\,x = -\,\Delta\,x^2\cos.\,x$

$\qquad d^3\,y = d\,(-\,w^2\cos.\,x) = +\,w^3\sin.\,x = +\,\Delta\,x^3\sin.\,x$

$\qquad d^4\,y = d\,(\quad w^3\sin.\,x) = +\,w^4\cos.\,x = +\,\Delta\,x^4\cos.\,x$

&c..

Therefore $\Delta\,(\cos.\,x) = d\,y + \frac{d^2\,y}{2} + \frac{d^3\,y}{2\cdot3} + \frac{d^4\,y}{2\cdot3\cdot4} + $ &c.

Or $\Delta\,(\cos.\,x) = -\,\Delta\,x\sin.\,x - \frac{\Delta\,x^2}{2}\cos.\,x + \frac{\Delta\,x^3}{2\cdot3}\sin.\,x +$

$\frac{\Delta\,x^4}{2\cdot3\cdot4}\cos.\,x - $ &c.

Example 17.—To find the increment of tan. x (radius unity). Put $w = d\,x = \Delta\,x$, a constant quantity, and $y = \tan.\,x$.

Then $dy = w \sec.^2 x$

$$\frac{d^2 y}{2} = w^2 \tan. x \sec.^2 x$$

$$\frac{d^3 y}{6} = \frac{w^3}{3}(2 \tan.^2 x \sec.^2 x + \sec.^4 x)$$

$$\frac{d^4 y}{24} = \frac{w^4}{3}(\tan.^3 x \sec.^2 x + 2 \tan. x \sec.^4 x)$$

$$\frac{d^5 y}{120} = \frac{w^5}{15}(2 \tan.^4 x \sec.^2 x + 11 \tan.^2 x \sec.^4 x + 2 \sec.^6 x)$$

&c. . . . .

Or by writing $1 + \tan.^2 x$ for $\sec.^2 x$, we have

$$\Delta(\tan. x) = dy + \frac{d^2 y}{2} + \frac{d^3 y}{2.3} + \frac{d^4 y}{2.3.4} + \&c. =$$

$$+ \; \Delta x \sec.^2 x$$

$$+ \; \Delta x^2 \tan. x \sec.^2 x$$

$$+ \; \frac{\Delta x^3}{3}(1 + 3 \tan.^2 x) \sec.^2 x$$

$$+ \; \frac{\Delta x^4}{3}(2 + 3 \tan.^2 x) \tan. x \sec.^2 x$$

$$+ \; \frac{\Delta x^5}{15}(2 + 15 \tan.^2 x + 15 \tan.^4 x) \sec.^2 x$$

$$+ \; \frac{\Delta x^6}{45}(17 + 60 \tan.^2 x + 45 \tan.^4 x) \tan. x \sec.^2 x$$

&c. . . . .

Example 18.—To find the increment of cot. x (radius unity). Put $w = dx = \Delta x$, a constant quantity, and $y = \cot. x$. Then by a process similar to that used in example 17, we have

$$\Delta(\cot. x) = - \; \Delta x \; \mathrm{cosec.}^2 x$$

$$+ \; \Delta x^2 \cot. x \; \mathrm{cosec.}^2 x$$

$$- \; \frac{\Delta x^3}{3}(1 + 3 \cot.^2 x) \; \mathrm{cosec.}^2 x$$

$$+ \; \frac{\Delta x^4}{3}(2 + 3 \cot.^2 x) \cot. x \; \mathrm{cosec.}^2 x$$

$$- \; \frac{\Delta x^5}{15}(2 + 15 \cot.^2 x + 15 \cot.^4 x) \; \mathrm{cosec.}^2 x$$

$$+ \; \frac{\Delta x^6}{45}(17 + 60 \cot.^2 x + 45 \cot.^4 x) \cot. x \; \mathrm{cosec.}^2 x$$

&c. . . . .

Example 19.—To find the increment of the sec. x (radius unity). By example 17,

$$\Delta(\sec. x) = \Delta x \tan. x \sec. x$$

$$+ \; \frac{\Delta x^2}{2}(1 + 2 \tan.^2 x) \sec. x$$

$$+ \; \frac{\Delta x^3}{3}(2 + 3 \tan.^2 x) \tan. x \sec. x$$

$$+ \; \frac{\Delta x^4}{3.4}(2 + 13 \tan.^2 x + 12 \tan.^4 x) \sec. x$$

$$+ \frac{\Delta x^5}{3.4.5} (28 + 87 \tan.^2 x + 60 \tan.^4 x) \tan. x \sec. x$$

$$+ \frac{\Delta x^6}{3.4.5.6} (28 + 317 \tan.^2 x + 648 \tan.^4 x + 360 \tan.^6 x) \sec. x$$

&c . . . .

Example 20.—To find the increment of cosec. x (radius unity).
By example 17,

$$\Delta (\text{cosec. } x) = - \Delta x \cot. x \text{ cosec. } x$$

$$+ \frac{\Delta x^2}{2} (1 + 2 \cot.^2 x) \text{ cosec. } x$$

$$- \frac{\Delta x^3}{3} (2 + 3 \cot.^2 x) \cot. x \text{ cosec. } x$$

$$+ \frac{\Delta x^4}{3.4} (2 + 13 \cot.^2 x + 12 \cot.^4 x) \text{ cosec. } x$$

$$- \frac{\Delta x^5}{3.4.5} (28 + 87 \cot.^2 x + 60 \cot.^4 x) \cot. x \text{ cosec. } x$$

$$+ \frac{\Delta x^6}{3.4.5.6} (28 + 317 \cot.^2 x + 648 \cot.^4 x + 360 \cot.^6 x) \text{ cosec. } x$$

Example 21.—To find the second increment of log. x.

By example 11, $\Delta (\log. x) = \frac{w}{2} - \frac{w^2}{2 x^2} + \frac{w^3}{3 x^3} - \frac{w^4}{4 x^4} + \&c.$

Where $w = d x = \Delta x$, a constant quantity; put $z = \Delta (l x)$

Then will $d z = - \frac{w^2}{x^2} + \frac{4 w^3}{x^3} - \frac{12 w^4}{x^4} + \frac{48 w^5}{x^5} - \frac{240 w^6}{x^6} + \&c.$

$$\frac{d^2 z}{2} = \ldots . + \frac{w^3}{x^3} - \frac{6 w^4}{x^4} + \frac{24 w^5}{x^5} - \frac{120 w^6}{x^6} + \&c.$$

$$\frac{d^3 z}{6} = \ldots . . . . - \frac{w^4}{x^4} + \frac{8 w^5}{x^5} - \frac{40 w^6}{x^6} + \&c.$$

$$\frac{d^4 z}{24} = \ldots . . . . + \frac{w^5}{x^5} - \frac{10 w^6}{x^6} + \&c.$$

$$\frac{d^5 z}{120} = \ldots . . . . - \frac{w^6}{x^6} + \&c.$$

Therefore $\Delta^2 (\log. x) = - \frac{\Delta x^2}{x^2} + \frac{5 \Delta x^3}{x^3} - \frac{19 \Delta x^4}{x^4} + \frac{81 \Delta x^5}{x^5}$

$$- \frac{411 \Delta x^6}{x^6} + \&c.$$

This series may be continued at pleasure, as the coefficients observe the following law, viz.

$2 \times 1 + 3 = 5, \; 3 \times 5 + 4 = 19, \; 4 \times 19 + 5 = 81, \; 5 \times 81 + 6 = 411, \&c.$

In finding the increments of examples 17, 18, 19, 20, it was necessary to use the differentials of the powers of the sines, cosines, tangents, secants, &c. I beg leave to insert the following table which I formed for similar purposes:

Differential Formulæ.

$d \, (\sin.^n x) = n \cos. x \sin.^{n-1} x \, d \, x$

$d \, (\cos.^n x) = - n \sin. x \cos.^{n-1} x \, d \, x$

$d \, (\text{vers.}^n x) = n \sin. x \, \text{vers.}^{n-1} x \, d \, x$

$d \, (\text{covers.}^n x) = - n \cos. x \, \text{covers.}^{n-1} x \, d \, x$

$d \, (\tan.^n x) = n \sec.^2 x \tan.^{n-1} x \, d \, x$

$d \, (\cot.^n x) = - n \, \text{cosec.}^2 x \cot.^{n-1} x \, d \, x$

$d \, (\sec.^n x) = n \tan. x \,.\, \sec.^n x \, d \, x$

$d \, (\text{cosec.}^n x) = - n \cot. x \, \text{cosec.}^n x \, d \, x$

Never having seen a general solution to the following problem, I have ventured to give the following, and will thank you to annex it to the foregoing.

Problem.—To find the *n*th increment of the log. *x*.
From the principles of increments, we have

$\Delta \; (\log. x) = \log. (x + w) - \log. x$

$$= \log. \left(1 + \frac{w}{x} \right) x - \log. x = \log. \left(1 + \frac{w}{x} \right)$$

$\Delta^2 \, (\log. x) = \log. (x + 2 w) - \log. (x + w) - \log. (x + w) +$
$$\log. x$$

$$= \log. (x + 2w) - 2 \log. (x + w) + \log. x$$

$$= \log. \left(1 + \frac{2 w}{x} \right) - 2 \log. \left(1 + \frac{w}{x} \right)$$

$$= \log. \left(1 + \frac{w}{x + w} \right) + \log. \left(1 - \frac{w}{x + w} \right)$$

$\Delta^3 \, (\log. x) = l \, (x + 3 w) + l \, (x + w) - 2 \, l \, (x + 2 w)$
$$- l \, (x + 2 w) + 2 \, l \, (x + w) - l \, x$$

$$= l \, (x + 3 w) - 3 \, l \, (x + 2 w) + 3 \, l \, (x + w) - l \, x$$

$$= l \left(1 + \frac{3 w}{x} \right) - 3 \, l \left(1 + \frac{2 w}{x} \right) + 3 \, l \left(1 + \frac{w}{x} \right)$$

$$= l \left(1 + \frac{3 w}{x} \right) - 3 \, l \left(1 + \frac{w}{x + w} \right)$$

In like manner may be found Δ^4 (log. *x*), Δ^5 (log. *x*), &c. Then by induction we conclude that

$$\Delta^n \, (\log. x) = l \left(1 + \frac{n \, w}{x} \right)$$

$$- n \, l \left(1 + \frac{(n - 1) w}{x} \right)$$

$$+ \frac{n \, (n - 1)}{2} \, l \left(1 + \frac{(n - 2) w}{x} \right)$$

$$- \frac{n \, (n - 1) \, (n - 2)}{2 \,.\, 3} \, l \left(1 + \frac{(n - 3) w}{x} \right)$$

$$+ \frac{n \, (n - 1) \, (n - 2) \, (n - 3)}{2 \,.\, 3 \,.\, 4} \, l \left(1 + \frac{(n - 4) w}{x} \right)$$

&c. . . . .

$w = \Delta x.$

Example 22.—To find the increment of the log. x.

Per theorem Δ (log. x) = log. $\left(1 + \frac{w}{x}\right)$; then per logarithms Δ

(log. x) = log. $\left(1 + \frac{w}{x}\right) = \frac{w}{x} - \frac{w^2}{2\,x^2} + \frac{w^3}{3\,x^3} - \frac{w^4}{4\,x^4} + $ &c.

See example 11.

Example 23.—To find the second increment of the log. x.

Per theorem Δ^2 (log. x) = $l\left(1 + \frac{w}{x + w}\right) + l\left(1 - \frac{w}{x + w}\right)$

But $l\left(1 + \frac{w}{x + w}\right) = l.1 + \frac{w}{x + w} - \frac{1}{2}\left(\frac{w}{x + w}\right)^2 + \frac{1}{3}\left(\frac{w}{x + w}\right)^3$
$- $ &c.

And $l\left(1 - \frac{w}{x + w}\right) = l.1 - \frac{w}{x + w} - \frac{1}{2}\left(\frac{w}{x + w}\right)^2 - \frac{1}{3}\left(\frac{w}{x + w}\right)^3$
$- $ &c.

Therefore Δ^2 (log. x) = $- \left\{\left(\frac{w}{x + w}\right)^2 + \frac{1}{2}\left(\frac{w}{x + w}\right)^4 + \frac{1}{3}\left(\frac{w}{x + w}\right)^6\right.$
$+ $ &c. $\left.\right\}$

(See Gentleman's Diary for 1818.)

The young algebraist will observe that the answer to example 11, as determined by Taylor's theorem, and that of example 22, are represented by the same series. But the answers to examples 21 and 23 are represented by series which are very different from each other, notwithstanding both are correct, and their law of continuation obvious.

ARTICLE IV.

On Urinary and other Morbid Concretions.
By William Henry, M.D. F.R.S. &c.[*]

THE analysis of urinary concretions is a subject of chemical research, which has already been investigated with so much ability and success, that it can now be expected to supply little more than a few scattered facts that may have escaped the industry of former inquirers. My experiments indeed were for the most part made more than 12 years ago; and such of the results as have not already appeared in an inaugural dissertation on the uric acid, printed in 1807, were reserved, as I there intimated, for a larger work on stone and gravel, which at that time I had it in view to undertake. This, if other impediments had not occurred to its execution, is now completely superseded by an excellent "Essay on the Chemical History and Medical Treatment of Urinary Calculi," lately published by Dr. Marcet.

[*] Read before the Medical and Chirurgical Society of London, March 2, 1819.

One effect of the delayed fulfilment of my purpose has been that
I have been anticipated by other writers as to several of the facts
that had occurred to me. Some of these, however, it may still
be proper to state in general terms, as they furnish additional
evidence on points that have been the subjects of differences of
opinion.

The urinary concretions from the bladder, which I have
examined experimentally, amount in all to 187. Of these, 34
were extracted by the late Mr. Ingham, of Newcastle-upon-
Tyne; 35 by the late Mr. White, of Manchester; 25 by Mr.
Hey, of Leeds; 23, partly by the late Mr. Gibson, and partly by
Mr. Ainsworth, both of this town; and the remaining 70 have
been given to me by surgical practitioners in this place,* and in
various parts of the kingdom, with the obliging view of facilitating
my inquiries. The number of the different varieties of calculi in
these several collections is stated in the following table, which
exhibits them in a sort of natural order, differing from the
chemical arrangement, which I have proposed in another place,
and which I still prefer for most of the purposes of classification.

TABLE I.

*Showing the Number of each Variety of Calculus in several
different Collections.*

	Chiefly uric acid.	Earthy phosphates.	Oxalate of lime.	Compound.	Cystic oxide.	Alternating Calculi.				Total.
						Uric acid and phosph.	Oxal. and pho. ph.	Oxal. and uric acid.	Oxal. ur. ac. and phos.	
Mr. Ingham's...	15	1	0	1	0	7	3	3	4	34
Mr. White's ...	19	7	3	0	0	3	2	1	0	35
Mr. Hey's	9	4	2	0	0	7	2	0	1	25
Mr. Ainsworth's	7	2	2	1	0	6	2	1	2	23
Own collection.	21	8	4	6	2	16	7	6	0	70
	71	22	11	8	2	39	16	11	7	187

From the preceding table, I have calculated the following,
exhibiting the proportion which each kind of concretion bears to
the whole number in the different collections. For example, in
Mr. Ingham's collection, the calculi consisting chiefly of uric
acid are to the whole number as one to one and two-tenths; in
Mr. White's as one to one and eight-tenths, the second decimal
figure being omitted throughout as unnecessarily minute.

* Especially by most of the late and present surgeons of the Manchester Infirm-
ary; and to Mr. Ransome, one of the surgeons of that charity, I am indebted for
dividing the calculi by a saw, without which division, collections of urinary con-
cretions can afford no useful information.

TABLE II.

Showing the Proportion of each Variety of Calculus to the whole Number in the different Collections.

Kind of calculus.	Ingham. Newcastle.		White. Manchester.		Hey. Leeds.		Ainsworth. Manchester.		Own.		Whole number.	
Chiefly uric acid	1 to	2·2	1 to	1·8	1 to	2·7	1 to	3·3	1 to	3·2	1 to	2·6
Earthy phosphates.	1	34·0	1	5·0	1	6·0	1	11·5	1	8·8	1	8·5
Oxalate of lime		—	1	11·6	1	12·0	1	11·5	1	17·7	1	17·0
Compound	1	34·0		—		—	1	23·0	1	11·8	1	23·5
Alternating.........	1	2·0	1	3·2	1	2·4	1	2·2	1	2·4	1	2·5
Alternating { Uric acid and phosphates	1	4·8	1	11·6	1	3·4	1	3·8	1	4·4	1	4·8
Oxalate and phosphates..	1	11·2	1	17·5	1	12·0	1	11·5	1	10·1	1	11·6
Oxalate and uric acid. ..	1	11·2	1	35·0		—	1	23·0	1	11·8	1	17·0
Uric acid oxalate and phosphates............	1	8·5		—	1	24·0	1	11·5		—	1	26·5
Cystic oxide		—		—		—		—		—	1	98·5

The greater proportion of uric acid calculi in Mr. White's collection than in that of Mr. Gibson and Mr. Ainsworth may, on first view, appear extraordinary, as those three gentlemen all practised surgery in the same town. It will be found, however, in the joint collection of the two latter, to be compensated by a much greater number of that variety of alternating calculus which is composed of uric acid and the phosphates in distinct layers. It may be remarked also that the greater number of Mr. White's operations were performed at a very remote period of time, when little interruption was given to the natural progress of the disease by the use of alkaline medicines, and that his patients were chiefly from a distance. Of late years, in consequence of the increase of public hospitals in the adjoining counties, cases requiring lithotomy occur comparatively very seldom at the Manchester Infirmary; and the town, and district immediately surrounding it, may be considered as remarkably unproductive of stone patients, though cases of gravel occur, so far as I have the means of judging, as frequently as in other remote districts.*

In the collection of Mr. Ingham, of Newcastle, the proportion of calculi composed entirely of the earthy phosphates is unusually small, but is compensated by the number of concretions in which the phosphates alternate with uric acid. On the whole indeed there is a remarkable uniformity in the composition of

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* It is a well ascertained fact, and one which should give encouragement to persons labouring under gravel, that this disease occurs very frequently without degenerating into stone; and that it is even endemic in districts where the stone is a very rare disease.—(See Beverwyk de Calculo, p. 78. Carleton de Lithiasi, p. 118, and Heberden Comment. p. 78.)

calculi generated in districts very remote from each other—a fact which proves that the causes rendering the stone endemic in certain countries act, not as was once imagined in supplying directly the material of which the concretions are composed, but in inducing a constitutional tendency to the disease.

It was the opinion of some of the older writers, that all calculous concretions (with the exception of such as are formed on extraneous substances accidentally introduced into the bladder) do in fact originate in the kidneys, and descending through the ureters, merely acquire an increase in the bladder by attracting solid matter from the urine. To this opinion, which Fernelius especially has ably supported,* it has been objected that stone in the bladder is in many instances not preceded by any pain in the region of the kidneys, or by the symptoms that denote the descent of a stone through the ureter.† It is perfectly conceivable, however, that a small calculus may find its way from the kidneys to the bladder without exciting pain in its passage. The opinion of Fernelius, and of others who agree with him, I find also to be confirmed by the appearance of almost all the calculi which I have ever examined, after having been divided by the saw ; for, except in very few instances, a central nucleus may be distinctly seen, sufficiently small to have descended to the bladder through one of the ureters, even when that passage has not been dilated beyond its natural diameter. The stone, therefore, is to be considered, essentially and in its origin, as a disease of the kidneys. Its increase in the bladder may be occasioned either by exposure to urine containing an excess of the same ingredient as that composing the nucleus, in which case it will be of uniform composition throughout ; or if the substance composing the nucleus should, either by a spontaneous change in the action of the kidneys, or by the effect of medicines, be secreted in less than natural proportion, the concretion will then, like any other extraneous body lodged in the bladder, acquire a covering of the earthy phosphates.

Under this view of the subject, it becomes highly important to ascertain of what ingredient the nuclei of urinary calculi are, for the most part, constituted, since it is in the tendency of the kidney to generate this ingredient that the primary cause of the disease must consist. Of the 187 calculi which I have examined, 17 have been formed round nuclei composed chiefly of oxalate of lime ; 3 round nuclei of cystic oxide ; 4 round nuclei of the earthy phosphates ; 2 round extraneous substances ; and in three, the place of the nucleus is supplied by a small cavity, occasioned probably by the shrinking of some animal matter, round which the ingredients of the fusible calculus had been deposited.‡ The remainder, amounting to 158, have a central

* Fernelii Opera, p. 317, folio.   † Beverwyk de Calculo, p. 69.
‡ Rau has shown, by a direct experiment, that pus may form the nucleus of an urinary concretion.—(See Denys de Calculo Renum, &c. p. 14.)

nucleus composed chiefly of uric acid. It appears then that in a very great majority of cases, the disposition to secrete an excess of uric acid has been the essential cause of the formation of stone; and it becomes important to inquire what are the circumstances that contribute to its excessive production, and by what plan of diet and medicine the tendency to its too great secretion by the kidneys may best be counteracted or removed. This inquiry, however, is not within the scope of the present essay, which is limited to the chemical composition of the concretions when actually formed.

Of Uric Acid Concretions.

It has never yet occurred to me to examine a calculus composed of this acid in a state of absolute purity. Of the concretions which I have classed under this head, a considerable number, after the action of pure potash, have left an insoluble residue of the earthy phosphates; and from the solution of those even, which have entirely dissolved in that menstruum, I have in no case been able to recover by the addition of acids, a quantity of uric acid equivalent to the weight of the calculus dissolved. The utmost that I have ever obtained has been 92 parts from 100 of an uric concretion. On this subject, therefore, my experience entirely agrees with that of Mr. Brande.* The loss doubtless arises from the decomposition of animal matter by the alkali. This, as I have stated in my Thesis, is partly urea, which I found may be separated by digesting the powdered calculus in alcohol, and evaporating the solution.† It is not, however, to urea that the colour of uric acid calculi is to be ascribed, but rather to the other substances which in urine generally accompany it; for it has been shown by Professor Berzelius and by Dr. Prout that pure urea is destitute of colour. In one instance only I have observed a vesical calculus composed chiefly of uric acid, to be of the whiteness of chalk; and from this the action of alcohol did not extract any portion of urea. Gelatine I have never been able to discover, by applying its appropriate test to water which had been digested in the powdered calculus; but the presence of albuminous matter appears to me to be indicated by light flocculi, which sometimes float over the uric acid, when precipitated by acids from its solution in alkali. It is probable, however, that the characteristic ingredient of urinary calculi does not necessarily require a cement to bind it together, but that the aggregative attraction of its particles is sufficient to unite them into a compact mass. All curative plans, therefore, which have in view the removal of a cementing ingredient (the mode in which Haller and Hartley explained the action of alkaline solvents) appear to me to be without probable grounds of success.

* Phil. Trans. 1808.   † Dissert. Inaug. 1807, p. 39.

Urate of ammonia, I believe with Mr. Brande, has been erroneously set down by Fourcroy and Vauquelin as an ingredient of urinary calculi. At least I have never found any indications of its presence in calculi which had been previously subjected to the successive action of alcohol and of acetic acid; menstrua, which would remove urea and the ammoniaco-magnesian phosphate, but would not, in the quantities employed, have separated urate of ammonia.

Several opportunities have been thrown in my way of examining urinary calculi, extracted from persons who had been long under a course of caustic alkaline lixivia. In one of these (No. 13 of Mr. Ingham's collection) the outer surface of the calculus might, on first view, have been supposed to have been eroded; but a closer examination satisfied me that the appearance was owing, not to the solution of the uric acid, of which the concretion chiefly consists, but to an irregular deposit of the earthy phosphates, occasioned probably by the medicine. Another calculus in the same collection (No. 15) taken from a person who had long been using Perry's solvent, was so brittle, that on attempting to divide it by the saw, it separated into concentric coats, composed of uric acid with a large proportion of the earthy phosphates. The third is a fusible calculus, now in my possession, of remarkable whiteness and compactness, and containing no appreciable portion of uric acid. In a fourth instance, a calculus, put into my hands by Dr. Brown, of Glasgow, which had been taken from a person after so free an use of alkaline medicines as to have injured his general health, consisted chiefly of the triple phosphate of ammonia and magnesia. It was so brittle that it broke almost into powder under the forceps, and was, therefore, extracted by the scoop. These cases, and others of the same kind, which I think it unnecessary to mention, tend to discourage all attempts to dissolve a stone supposed to consist of uric acid, after it has attained considerable size in the bladder; all that can be effected under such circumstances by alkaline medicines appears, as Mr. Brande has remarked,* to be the precipitating upon it a coating of the earthy phosphates from the urine, a sort of concretion which, as has been observed by various practical writers, increases much more rapidly than that consisting of uric acid only. The same unfavourable inference may be drawn also from the dissections of those persons in whom a stone has been supposed to be dissolved by alkaline medicines; for in these instances it has been found either encysted or placed out of the reach of the sound by an enlargement of the prostrate gland. The former source of fallacy was shown to have existed even in one of the cases which procured to Mrs. Stevens the parliamentary reward of 5000*l.*; †

* Philosophical Transactions, 1808.
† Newman's Inquiry into the Merits of Solvents, London, 1781.

and examples of the latter kind have been related by Dr. Heber-
den and Sir Everard Home.

Two instances have fallen within my knowledge, in which
persons have voided quantities of uric acid with the urine, far
exceeding any thing that I can find upon record. In the first,
which was mentioned to me by Professor Monro, of Edinburgh,
every pint of the urine voided by a man about 40 years of age,
who laboured under symptoms of gravel, deposited about two
ounces of a brick-coloured sediment, which I found on examina-
tion to be chiefly uric acid with a very small relative proportion
of the earthy phosphates. In another instance, a lady of middle
age, who was subject to gravel, was in the habit, when warned of
its approach by the usual symptoms, of having recourse to a
medicine, the composition of which is kept secret, but which
appears to me to be nothing more than spirit of turpentine
coloured by a little petroleum, with the addition of a portion of
tincture of opium. The uniform effect of this medicine was the
discharge of a sandy substance in such quantity that more than
four ounces were sometimes voided within the space of two or
three days. It was composed chiefly of uric acid, with a small
proportion of urea and of the earthy phosphates. I have since
known another instance in which the same medicine has produced
a similar effect, though not to an equal extent, probably by acting
as a stimulant to the kidneys, and clearing them by the increased
flow of urine which it excites of the sand that had been depo-
sited in the tubuli uriniferi and pelves of those organs.

Calculi composed chiefly of the Earthy Phosphates.

The pure phosphate of lime, or bone earth calculus, I have not
been able to recognize in any of the collections of calculi which
I have examined, though assisted by a recollection suffi-
ciently distinct of one which was shown to me some years
ago by Dr. Wollaston ; nor have I ever found the triple phos-
phate of ammonia and magnesia composing, in a pure state, an
entire calculus, though in Mr. White's collection there is one
containing more than 90 per cent. of that salt. From this pro-
portion I have found it in a variety of others, down to 20 and
even 10 per cent. With phosphate of lime, in proportions which
seem to have a considerable range, it constitutes the fusible
calculus, and this mixture forms the principal ingredient of
calculi that have concreted round foreign substances. A calculus
in Mr. White's collection, the nucleus of which is a bougie that
had slipped into the bladder, is composed of

Phosphate of lime	20
Ammoniaco-magn. phosph.	60
Uric acid.	10
Animal matter.	10
	100

`In four instances only out of 187, the calculus has been composed throughout of the earthy phosphates; and in these I have not been able to discover a nucleus of any other substance. I consider the fact, therefore, as sufficiently established, that in some instances, though comparatively very few, a tendency to secrete the earthy phosphates in excess is a cause of the formation of stone, first probably in the kidneys, and subsequently in the bladder. This tendency indeed, as is well known, sometimes manifests itself by the discharge of urinary gravel, consisting of the triple phosphate either alone or in conjunction with phosphate of lime.

Several years ago, the Rev. Mr. R. of Cheadle, in Staffordshire, consulted me respecting a train of very distressing symptoms, some of which evidently denoted considerable disease in the kidneys. His urine, which at some times was perfectly limpid, was at others loaded with a white substance, which gave it, when first voided, the opacity of milk. On standing, a copious deposit took place, a portion of which was sent to me for examination. It was perfectly white, and so impalpable as to resemble a chemical precipitate. On analysis, it proved to consist of nearly equal parts of the triple phosphate and phosphate of lime. The discharge of this powder was always preceded by violent attacks of sickness and vomiting, and its quantity was invariably increased whenever he took soda water or any other alkaline medicine. Beside the affection of the kidneys, there appeared to me to exist important disease of the chylopoietic viscera, and to this I ascribe his death, which took place a few months afterwards. In this case it was remarkable that the weight of the body was reduced from 183 to 100 pounds at rather an early stage of the disease, without a corresponding degree of muscular emaciation, owing obviously to the imperfect nutrition of the bones, in consequence of the waste of the phosphate of lime through the urinary passages.

Mulberry Calculus.

In calculi of this description I have always found, with Dr. Wollaston and Mr. Brande, an admixture of other substances with the oxalate of lime, which is to be considered as their characteristic ingredient. One of the best marked specimens I have ever seen of the rough kind afforded, from 10 gr. 5·3 gr. of carbonate of lime, equivalent to nearly 6·6 of oxalate, 1 gr. of uric acid, 0·3 gr. of phosphate of lime, and a quantity of dark coloured flocculi of animal matter, which did not descend along with the uric acid, when the latter was precipitated from its alkaline solution. These flocculi were soluble again in pure potash, but not in alcohol or in dilute acids. The colouring ingredient of this variety of calculus is communicated both to caustic alkalies and to concentrated muriatic acid, the latter of which becomes tinged, like a strong infusion of roasted coffee. On diluting the solution, part of the oxalate of lime is deposited, but the colour-

ing matter remains dissolved. It is probably derived originally from effused blood, for the smooth variety of calculus which consists chiefly of oxalate of lime, is not distinguished by this dark shade of colour.

Cystic Oxide Calculi.

By means of Dr. Wollaston's clear description of this rare variety of calculus, I have recognised two specimens of it in my own collection, but with the histories of both I am wholly unacquainted. They have obviously been extracted from the bladder; the one, when entire, weighing 660 gr. and the other 334. In each, the nucleus is the same substance as the rest of the concretion; and in a third specimen, also in my possession, a very small spherule of cystic oxide forms the nucleus of a moderately sized calculus, the rest of which consists of uric acid. This oxide appears, therefore, as Dr. Marcet has properly remarked, to be in reality the production of the kidneys, and not, as its name would import, to originate in the bladder.

Calculi, the Ingredients of which are disposed in alternate Layers.

. Of these I have little more to observe than will be suggested by inspecting the first table; viz. that calculi composed of layers of uric acid and the earthy phosphates are, in the collections which I have examined, the most frequent variety of the alternating kind; next follow those of oxalate of lime and the phosphates; then concretions of oxalate of lime alternating with uric acid; and lastly, those occur most seldom in which the three substances just mentioned alternate together. I have not met with an instance of a calculus containing four ingredients in distinct layers; and it appears, from the testimony of others, that such examples are extremely uncommon.

Foreign Substances voided in the Urine.

I have related, in a periodical medical journal,* the case of an elderly gentleman, who discharged in his urine the larvæ of an insect, which, when first voided, were alive and vivacious, and so far as could be made out by an eminent naturalist, belonged to some species of the coleopterous order. In this case, though the patient would not consent to be sounded, there was doubtless a stone in the bladder, and, as appeared to me, extensive disease of the bladder itself and of the prostate gland; but no examination I believe, was made after his death, which was occasioned suddenly by a fit of apoplexy.

I have lately been made acquainted, by a gentleman of middle age, with a singular discharge which he frequently observes in his urine, of a considerable number of short hairs. Besides that he is above all suspicion of being deceived himself, or deceiving others, I have satisfied myself, by the most careful investigation,

* Edin. Med. Journ. vii. 147.

that they have their origin from the inner surface of the bladder, or from some of the urinary passages. They are of various lengths, from one-tenth of an inch to an inch, and now come away without giving him any uneasiness, though he has at times suffered pain from the discharge of gravel of the uric acid kind. On one occasion, the hairs which were voided had acquired, before their discharge, a distinct coating of uric acid. The symptoms having at one time excited suspicion of a stricture of the urethra, a bougie was twice introduced without giving him pain ; nor was its use followed by any increase of the number of hairs that were voided, which might perhaps have been expected, if they had grown from the membrane of the urethra.*

Of Morbid Concretions from other Parts of the Body.

Pulmonary Concretions.—A pulmonary calculus, expectorated several years ago by a patient of the late Dr. Ferriar, was found to be chiefly composed of phosphate of lime, with a very minute proportion of carbonate. Such also has been the composition of other specimens, given to me by Dr. Baron, of Gloucester ; but a remarkably large one in the possession of Mr. Ainsworth, which weighed, when entire, 51 gr. and exhibited a complete cast of one of the bronchial cells, is principally composed of the triple phosphate, with a very small relative proportion of phosphate of lime, and a mere trace of carbonate. Some concretions taken from the lungs by the late Mr. Allan Burns, of Glasgow, have their earthy part composed of about one-fifth of the triple phosphate, and four-fifths of phosphate of lime, with a minute proportion of the carbonate of that earth. The subject from whom these concretions were taken after death, I was informed by Mr. Burns, was a female about 15 years old, who, though affected with violent cough, had never expectorated any calculous matter. The spine in this case was so much incurvated that, towards the close of life, the face approached nearer to a horizontal than to a perpendicular line. The substance of the lungs when grasped was felt to be full of hard knots, from the size of a pea to that of a hazel nut ; and a concretion, about the size of a large musket-ball, was found firmly impacted in the left branch of the trachea, near to its origin. By a care-

* When examined chemically these fibres do not appear to differ from common hair ; but it has been observed by Dr. Wollaston, that they differ in some respects in their mechanical texture, since they have not that slight roughness in one direction of the surface on which the *felting* property of common hair of every kind depends. This property of hair is most distinctly shown by pressing it between the fingers, and at the same time sliding the fingers upon each other in the direction of the hair, which will by this motion be seen to travel forward with its root foremost. The finger which moves *from* the root slides freely along the hair, while the other finger is prevented from sliding in the opposite direction by a degree of roughness (which is thus sensible though not in any way visible) but in its turn this finger also will move from the root while the hair now rests against the opposite finger. It has also been remarked by Dr. Wollaston, that common hairs are not really tubular, as has often been asserted, but that these fibres really are so.

ful examination of the concretions in the substance of the lungs,
Mr. Burns ascertained that each was lodged in a bronchial cell,
and was enveloped in a distinct capsule, which admitted of
being readily separated from the membrane of the air cell.
Indeed in all concretions discovered in the soft parts of the body,
Mr. Burns informed me that he has uniformly found a peculiar
substance containing the solid substance, and over this another
sheath of dense membrane. The inner covering he supposed to
belong essentially to the concretion, and the outer one to be
formed in consequence of the irritation caused by the presence
of an extraneous body.

Calculi from the Spleen.—For the opportunity of examining
these, I was indebted to the same zealous and able anatomist.
They were of small size, shaped like a pear, of a yellowish-white
colour, and were composed of bone earth, without any portion
of the triple phosphate.

Small Crystals formed on the Surface of a cancerous Prepara-
tion, kept in Spirit of Turpentine.—These also I received from
Mr. Burns ; and though not strictly belonging to the class of
morbid concretions, I mention them here on account of their
singular composition. They are in very minute parallelopipe-
dons, are fusible when placed on a piece of iron heated below
redness, and evaporate in an aromatic smoke. They are very
sparingly soluble in water, but more so in alcohol; and the latter
solution, when concentrated, reddens litmus paper. They agree,
therefore, in their properties with the camphoric acid, and fur-
nish an instance of the production of that acid under circum-
stances not before observed. Whether they had passed through
the intermediate state of camphor, which, by well known
treatment, may be obtained from spirit of turpentine, it is now
impossible to ascertain. Mr. Burns, however, assured me that
they may not unfrequently be seen on preparations kept in that
fluid ; but never, except when the parts have been imperfectly
dried before being immersed in it. It is probable, therefore,
that they may be found under similar circumstances in other
anatomical collections.

Manchester, Dec. 16, 1819.

ARTICLE V.

*Memoir on Sulphuric Ether.** By John Dalton.

IN my essay on the force of steam, read before the Society in
1801, and published in the fifth volume of the Memoirs, I stated
some experiments on the force of vapour from sulphuric ether,

* Read before the Literary Society at Manchester, April 16, 1819.

at different temperatures, as exhibited in a Torricellian vacuum, also the force of the same when admitted into a limited portion of air. From these experiments, as well as from corresponding ones made with water, alcohol, and other fluids, I was led to adopt the important conclusion, that steam acquires the same force in air as in a vacuum, and that it ought to be considered the same independent fluid in both cases. Consequently if p denote the pressure of any given volume of air (1), and f denote the pressure of steam of a given temperature, such steam being admitted to the air, the volume of both in due time becomes $\frac{p}{p-f}$.

This theorem is most beautifully illustrated by sulphuric ether. Let a common barometer have a drop of ether let up into the vacuum; it will instantly depress the mercury several inches, more or less according to the temperature. Suppose it were 10 inches, the barometer being 30; then $\frac{p}{p-f} = 1·5$; that is, if ether be passed up into air under those circumstances, it will in due time increase the volume of air 50 per cent.

For six years after this I was occasionally engaged in the further investigation of the nature and properties of ether, in which several additional facts, and some corrections of those antecedently announced, occurred. The combustion of ether was effected in various ways, as well as its analysis, by heat and by electricity.

During all this time I procured my ether in small quantities at a time, and of various druggists, as suited my convenience. Once or twice I ascertained the specific gravity of the article to be at or near 0·75; and I never found reason to suspect there was much difference in the specimens. Occasionally when great part of the ether was evaporated by time and neglect, I found a few drops at the bottom of the phial, which did not possess the properties of ether, but this was too small to be much regarded. In an excursion to Edinburgh and Glasgow in 1807, I exhibited the steam of ether, as above described, to a few persons in those two places; when at the latter place, Dr. Ure was so good as to supply me with ether, but upon trial it did not present the properties I had usually recognized, which at the time I attributed to accidental impurities, acquired in the laboratory; upon this he accompanied me to a druggist, where I was immediately supplied with ether of the requisite purity. I apprehend Dr. Ure's ether must have been the *spiritus ætheris sulphurici* of the Edinburgh college, made by adding two parts alcohol to one of ether; or perhaps ether not rectified.

In 1808 I published the first part of my New System of Chemical Philosophy, in which I digested all the knowledge I then had on the force of steam from ether in a tabular form. I had acquired from actual observation the forces in a range of tem-

perature from 0° to 212°. In my former publication I had
concluded that the variations in the force of steam from water
and ether were the same for the same intervals of temperature;
that is, if the force of steam from water was diminished from 30
to 15 inches of mercury, by a diminution of temperature of 30°;
then that of ether would be diminished from 30 to 15 inches by
the same number of degrees, though in a much lower part of the
scale; the former being from 212° to 182°, and the latter from
98° to 68°. Subsequent experience, however, led me to appre-
hend that the above intervals of temperature, though expressed
by equal expansions of mercury, are not in reality equal intervals;
but that equal intervals are rather denoted by the forces of steam
being in geometrical progression. Consistent with this view I
found tl at steam from water and ether would concur, for a long
range of temperature, with the difference of ratios only; that of
water being 1·321 for 10° of temperature, whilst that of ether was
1·2278.

In the above work occurs the following observation: " Ether,
as manufactured in the large way, appears to be a very homoge-
neous liquid. I have purchased it in London, Edinburgh,
Glasgow, and Manchester, at very different times, of precisely
the same quality in respect to its vapour." This observation,
though warranted from my limited experience at the time, I
now find not altogether correct; I am sorry that it has occa-
sioned an ingenious experimentalist to be led into a labyrinth of
error.

The bulk of the ether used in this country has I find of late
years been prepared by one manufacturing house in the neigh-
bourhood of London. Three qualities of the article are made
according to the different uses intended. The highest quality is
only made for particular purposes, and is, therefore, not very
commonly met with; it is about 0·73 specific gravity; the
second quality is that intended for medicine; it is of 0·75 speci-
fic gravity, and is that with which all the country druggists and
apothecaries are or ought to be supplied as a standard uniform
article; it is that which I have always met with in the shops, and
which I have taken for genuine ether in my former experiments.
The third quality is of the specific gravity 0·78 or 0·79 usually;
of course it is much inferior to the last in purity. But it may be
proper to observe, that this is the first state of the other two
qualities; they being produced from this by ulterior processes
called *rectification*.

It is well known that sulphuric ether is procured by distilling
a mixture of sulphuric acid and alcohol. The proportions
usually prescribed are equal weights of concentrated acid and
alcohol. By due management, a liquid of the specific gravity
0·785 or 0·79 is obtained, called ether. It is the ether of the
third quality, just mentioned, and is in fact a compound of

alcohol and ether chiefly, in proportions to be investigated here-
after. In this state it is usually called *unrectified* ether.

When this last liquid is redistilled by a moderate heat till one
half has passed over, the liquid in the receiver is denominated
rectified ether. It is usually about 0·75 specific gravity, corre-
sponding to the *second* quality. It still consists of ether and
alcohol, but with much less alcohol than before. There is great
reason to believe that both the *unrectified* and *rectified* ether, as
thus prepared, are destitute of water, except so far as it is an
essential element of the two liquids, ether and alcohol in their
purest states ; the sulphuric acid being well able to retain all the
excess of water of common alcohol in the temperatures employed
in the two distillations.

Ether of the first quality, or that in its purest state is to be
obtained from the rectified ether just mentioned. The object is
to abstract the alcohol still remaining in the rectified ether.
This may be done in great part by repeated distillations ; always
taking the first produce and setting aside the remainder for other
use ; but this method is tedious and expensive. A more ready
method is to agitate the rectified ether with about its own bulk
of pure water; after agitation the mixture resolves into two
fluids, a heavier and a lighter; the lighter may be decanted, and
will be found about two-thirds of the volume of ether used ; it
will have the specific gravity 0·73 nearly, and may be considered
as ether of the first quality. But it is demonstrable that it still
contains *some* alcohol, and has besides acquired a portion of
water from this process. The watery stratum below contains
the greatest part of the alcohol, and has also taken along with it
a portion of ether, as is evident from the smell, which is much
the same as that of ether itself. This heavy liquid has the
specific gravity of 0·96 or 0·97 usually. If this ether of 0·73
specific gravity be again treated with water, it will be reduced
nearly to 0·72 specific gravity ; but it still contains minute por-
tions of both alcohol and water, the quantities of which are not
easily appreciated. Subsequent distillation would doubtless
improve the quality a little ; but for most practical purposes
there is reason to believe that no material difference would be
found between the above and ether of absolute purity.

Having obtained ether of the specific gravity 0·72, and alcohol
of 0·83 specific gravity, both of which may be considered as very
nearly pure or free from water; mixtures of these two liquids
may be made in any proportions, and the resulting specific
gravities ascertained ; from which we may be enabled to esti-
mate the proportions of the two fluids in any specimen where no
water is present.

This operation, however, is more difficult than may be
imagined. By taking ether and diluting it successively with
equal portions of alcohol, the resulting specific gravities may be

found in the usual way, provided we could guard against any loss of the mixture. But such is the evaporating power of ether, especially when pure, that it is impossible to pour it from one vessel into another in the open air without much loss. In one instance I found that after six successive dilutions and 12 transfers, made with great care, I had lost one-fifth of the whole weight used. In such case, if the diluting portions are not diminished duly, the results must be erroneous. One circumstance is favourable, the increase of density by chemical action appears to be very small ; so that the densities may be *calculated* without very material error. The following table will afford a moderately good approximation, which may have its use till a better is made.

Table of the Specific Gravities of Mixtures of Ether and Alcohol.

Ether.		Alcohol.	Sp. Gr.
100	+	0	0·720
90	+	10	0·732
80	+	20	0·744
70	+	30	0·756
60	+	40	0·768
50	+	50	0·780
40	+	60	0·792
30	+	70	0·804
20	+	80	0·816
10	+	90	0·828
0	+	100	0·830

From this table it would seem that ether of the second quality, or that of the shops in general, contains about 25 per cent. of alcohol ; and that of the third quality from 55 to 60 of alcohol ; and the proportion of this article will be still greater on the probable supposition that pure alcohol is as low as 0·82 in specific gravity.

So far we have considered the mixtures of ether and alcohol in their purest states, or nearly such ; and it has been observed that in the ordinary course of manufacture, it is these mixtures, only varied in proportion, that occur. But if we introduce water so as to vary the proportions of ether, alcohol, and water, indefinitely, then some new phenomena occur, and the quantity of ether in such mixtures is no longer to be determined by the specific gravities. These mixtures are in some proportions uniform throughout ; in others, they resolve into two fluids of different specific gravities, alike transparent and colourless, but easily distinguishable from a filmy-like surface between the two fluids. Both the heavy and light, or as they may be called, the watery and ethery fluids, contain in all cases less or more of all the three ingredients. They seem to vary in their specific gravities according to this law ; whenever the upper fluid is extremely light, the under one is extremely heavy ; namely,

about 0·72 and 0·98 respectively; and whenever the under fluid is extremely light, then the upper one is extremely heavy, but the two never approximate nearer than 0·93 and 0·82 respectively. As far as I have found, I am pretty well convinced that in this last case the heavy fluid is constituted of 1 atom of ether, 1 of alcohol, and 5 of water; and the light fluid of 1 ether, 1 alcohol, and 1 water, being a true ternary compound of the three elements. These facts are beautifully exhibited by a single experiment. Let equal volumes of pure ether and water be agitated together; on subsiding, the very heavy and very light fluids are immediately perceived; let then pure alcohol be added by degrees, and agitated; it will be observed that both fluids have increased in volume upon each addition, till at length the upper fluid arrives at its maximum volume and specific gravity. A further addition of alcohol then diminishes the volume of ether till at length it disappears, and the whole becomes one uniform fluid.

The boiling point of ether I find forms a curious part of its history; I mean that point of temperature when its vapour is of sufficient force to balance the weight of the atmosphere. In my early experiments I found the point by immersing a thermometer in the boiling fluid, when it stood at 102°; but in subsequent experiments I used a barometer tube bent about one-third from the sealed end, and the legs laid parallel. A small portion of ether was let up to the sealed end, and the tube from thence to a little past the turn was filled with mercury. The instrument thus prepared was immersed in a tall jar of warm water till the vapour arose from the ether and depressed the mercury, which ascending in the other leg, was brought to a level in the two. In this way, the same ether, in the temperature of 98°, exhibited a force equal to the atmosphere. Something like this I find takes place in alcohol of 0·83 specific gravity. It boils in a phial at 176°; but in a tube its vapour is equal to the atmosphere in a temperature of 172°. Pure ether of 0·72 specific gravity boils in the tube at 95° or 96°, as Gay-Lussac has observed; but in a phial I find the thermometer may be raised to 98° in the boiling liquid. The boiling point of a mixture of pure ether and pure alcohol may be made to vary from 96° to 170°; but we cannot infer the boiling point from a knowledge of the proportions of the mixture; it is always much nearer that of ether than the proportions would indicate. Indeed it is the same with alcohol and water, and all similar mixtures. A mixture of equal parts of alcohol and water boils at 183°; whereas by the rule of proportion it ought to boil at 194°. A mixture of four parts ether and three parts alcohol I found boiled at 117° in the tube, and 122° or 123° in the air, which by proportion should have boiled at 127°. It was of specific gravity 0·769, and might, therefore, be considered as between the second and third quality.

The modifications of the boiling point of ether produced by

water, however, are the most astonishing. The heavy fluid aris-
ing from the washing of ether by water, which is of the specific
gravity 0·96, and which consists of 8 or 10 parts of water and
1 or 2 of ether and alcohol, boils at 103° in the tube; but if the
temperature be increased, it soon ceases to manifest the increas-
ing progressive elasticity of pure ether, as may well be expected.
The reason of this is pretty obvious; water possesses little or no
affinity for ether; it yields readily the few atoms it possesses to
the influence of heat, and when they are raised, the supply
ceases. Hence we see the necessity of using a pure ether when
the tension at various successive temperatures is to be found.

Specific Gravity of Ether Vapour.

In 1803 and 1804 I made a great many experiments on the
combustion of ether vapour mixed with oxygen gas by electricity.
These sufficiently demonstrated the great specific gravity of this
vapour, as it was sufficient to have four or five per cent. of
volume of it to produce abundance of carbonic acid, and to
require a greater abundance of oxygen. I found it expedient to
ascertain as near as possible the exact specific gravity, and
attempted it as follows in September, 1803.

I took a balloon glass, of the capacity of 253 cubic inches,
having a wide neck, to which was adapted a brass cap and stop-
cock. Into this a graduated tube, $\frac{3}{10}$ths of an inch diameter,
containing ether of 0·758 specific gravity, and a manometer
were introduced; the manometer was as usual a tube of $\frac{1}{15}$th
inch bore, closed at one end and duly graduated, with a globule
of mercury sliding in it. The vessel was immediately made air
tight, and kept so for several days, during which time the pro-
gress of the evaporation and of the gauge was occasionally
noted. The temperature of the air in the room was usually
about 55°; but as this was of no importance, it was not particu-
larly noted. The observations follow : the ether tube was gradu-
ated into water grain measures : barometer 30 inches.

			Manometer.	Measures of ether evaporated.
Sept.	23	2 p. m.	885	0·0
		5 ——	868	6·5
		8 ——	858	10·0 —
	24	1 a. m.	848	16·5
		9 ——	830	20·5
		3 p. m.	825	24·6
		10 ——	818	28·5
	25	1 p. m.	800	34·0
		12 ——	795	38·9
	26	2 p. m.	790	42·0 —
		9 ——	780	46·5
	28	9 a. m.	772	49·5

Now 49·5 measures of ether = 37·5 gr. and this quantity being by the manometer = $\frac{113}{772}$ of the atmospheric pressure, we have 113 : 37·5 :: 772 : 256 gr. the weight of 253 cubic inches of ethereal vapour of atmospheric force ; but the weight of the same volume of common air = 77 gr. Hence ethereal vapour = 3·3 times the specific gravity of air.

I find amongst my notes in 1805 a similar experiment, from which the specific gravity was deduced = 2·65 only. This difference occasioned me to repeat the experiment as follows :

Balloon containing 404 *Cubic Inches* = 123 *Gr. of Air.*

Barometer, 30 inches.
Ether, 0·728 specific gravity in the temperature 48°.

			Manometer.	Measures of ether evaporated.
1819.—Feb. 25	10	a. m.	4100	0·0
	1	p. m.	4052	8·0 —
	2¼ ——		4040	9·5
	3 ——		4025	11·0 —
	5¼ ——		4000 +	13·5
	7 ——		3990	15·0
	9 ——		3966	17·0
26	9	a. m.	3908	25·0
	11 + ——		3903	27·0 —
	2	p. m.	3900 —	28·0
	6 ——		3881	30·0
	9 ——		3870	32·0
27	9	a. m.	3824	36·0
	9	p. m.	3812	39·0

At this period the cock was turned, and the air and vapour let out, till the equilibrium was restored with the atmosphere, the barometer being then 29·5 ; the thermometer was not noted. In a few minutes the cock was again turned, and the experiment continued.

			Manometer.	Measures of ether evaporated.
Feb. 27	9½	p. m.	4077	0·0
28	2 ——		4040	5·5
	9 ——		4025	7·5
March 1	9	a. m.	4006	10·0
	9	p. m.	3985	14·0
2	9	a. m.	3969	15·5
	9	p. m.	3950	18·0 —
3	9	a. m.	3937	19·5
	9	p. m.	3919	22·0
4	9	a. m.	3908	24·0
	9	p. m.	3890	25·0

			Manómeter.	Measures of ether evaporated.
March 5	9	a. m.	3885	27·0
6	9	——	3874	28·0
	9	p. m.	3874	28·0+
7	9	a. m.	3874	28·0+

For the last two days there was only a drop of fluid left at the bottom of the tube (nearly five inches deep) which seemed to be not evaporable; but it was judged proper to continue the experiment in order to ascertain whether the vessel was perfectly air tight, and of course the gauge would continue stationary. The drop of fluid smelled of alcohol, and when diluted and treated with muriate of barytes was milky.

By making the calculation as above, the specific gravity of ether vapour from the first part of the experiment comes out 3·05, and from the last part, 3·2. The slow manner in which ether evaporates in these circumstances is surprising; in the latter part of the experiment it is to be ascribed to the depth of the surface of fluid in the tube, and the partly saturated air.

Though convinced the above results were very good approximations, I was desirous to have a confirmation of it by some more direct method. I took a bottle of the capacity of 2,600 gr. of water, and graduated accordingly; this being filled up to 1,100 gr. with dry mercury was inverted in the mercurial trough with 1,500 common air. Through this mercury were passed 1, 2, 3, or more grains of ether, which expanded the air, and from the quantity of expansion, compared with the weight of ether let up, the specific gravity of the vapour was inferred. This method did not give uniform results owing to a considerable portion of such minute quantities of ether being entangled by the mercury in its passage. To remedy this, I took a small tube, one-seventh of an inch in diameter internally, and two inches long, which was sealed at one end, and then graduated into water grains, which was such as to allow nearly one-fourth of an inch for one grain. This was filled with mercury, except for one, two, or more grain measures, which were afterwards filled with ether, and the finger being applied, the tube was plunged into the mercury and passed through the neck of the bottle up to the surface of the mercury in the bottle. In this way the ether was conveyed through the mercury without quitting the tube, and by gentle agitation was ejected and dissipated in vapour in a few minutes afterwards. The results in several experiments were nearly uniform, giving an increase of volume of gas from 255 to 275 grain measures for each grain of ether in weight. This gives the specific gravity of ether vapour from 3·1 to 3·3. On the whole, I think 3·1 is probably the nearest expression in two places of figures that can be attained.

Elasticity of Ether Vapour, the same in Air and in a Vacuum.

The same tension or elasticity of ether vapour takes place in air as in a vacuum, just as with the steam of water and other liquids. But this is not true of impure ether, if it be made to pass through water into the air, because by this operation it is improved in quality, though greatly diminished in quantity.

When the temperature of the air was 43°, and barometer 29·70, I passed up through water into a graduated tube containing 51 grain measures of air, about three or four grains of 0·73 ether. The air was in a few minutes expanded to 74 measures; and the ether barometer (that is, a barometer with the same kind of ether thrown up into the vacuum) stood at 20·5 in the same temperature; hence we have $\frac{29\cdot7}{20\cdot5} \times 51 \left(\frac{p}{p-f} \times 51 \right) = 74$

nearly; which accords with the before-mentioned theorem. The tube being afterwards immersed in water of 66° gave 104 measures of vapourized gas; and in 70° gave 118 measures. It stood for some months in water, still retaining a fluctuating volume of gas, according to the changes of barometer and thermometer; and at last the gas was passed through water, and instantly gave the original 51 measures of air.

The quality of ether may be judged of from passing a small portion of it through water in a graduated tube. Thirty grain measures of the best ether (0·73) passed up a tube of eight inches long filled with water lost four or five gr. Thirty grains of another ether, consisting of a mixture of 15 ether (0·735) and 15 alcohol (0·85), when passed in like manner, only gave five measures of fluid ether, swimming on the surface of the water.

Relation of Ether Vapour to Liquids.

Gases vapourized by ether may be kept over dry mercury, and transferred through the same without loss. But they are not kept over water, alcohol, and other liquids, without loss of vapour, though this is variable according to the nature of the fluid and other circumstances.

Alcohol absorbs ether vapour out of air much faster than water does. I filled two similarly graduated tubes with etherized air, and placed them over alcohol and water respectively: they lost vapour as under:

Tube over alcohol.	Tube over water.
155 measures.	155 measures.
116 in 5 minutes.	142 in 10 minutes.
112 in 8 minutes.	138 in 13 minutes.
104 in 30 minutes.	130 in 30 minutes.
100 washed.	100 washed.

The non-efficiency of water in abstracting ether vapour is further manifested by the following experiment.

I took a tall graduated cylindric jar, of three inches diameter, into which 20 oz. measures of air were passed over water. Thirty grain measures of ether (0·73) were then passed up into the air, through a volume of five inches of water, which was of course diminished a little in its passage, and then spread over the surface of the water to the thickness of $\frac{1}{200}$th of an inch nearly. The volume of air and vapour varied as under :

H.	M.		Oz.
—	—	....	20
—	3	....	22¼
—	6	....	24
—	12	....	26
—	20	....	27¼
—	26	....	28¼
—	32	....	28¼
—	50	....	28
2	27	....	27
1 Day		....	23
1 Week		....	21¼
		Washed.	20

Here it is observable, the vapour increased for half an hour, and then began to decline again, but slowly. It increased the volume by 8¼ oz. = 3,960 grain measures, which is equal to 15 gr. in weight by the preceding determination; but the ether weighed 22 gr.; so that a loss of one-third of the weight of the ether only was occasioned by the action of so great a surface of water on it for half an hour.

Force of Ether Vapour.

My former experiments on the force of ether having been made with an article not of the highest purity, they ought all to exhibit a force *too low* for the temperature. Such I find to be the fact; at least within a range of temperature of easy investigation, that is, from 30° to 140°. The difference, however, is but small, and may, without much error, be corrected by deducting 2° or 3° from the respective temperatures, as given in my table. (New System of Chemistry, p. 14.) The apparatus to be used, consists of a common barometer tube, one bent into a syphon at one-third of the length from the sealed end, and a tall smaller one bent six or seven inches from the sealed end, and having the other leg 40 inches long. The first of these instruments is best used for atmospheric temperatures, having a drop of ether let up into the vacuum. The second, is to have its short leg filled with mercury, and an inch of the other leg, a drop of ether being at the top of the mercury in the short leg.

This is used from temperature 80° to 110° or 120°. The third is to have its short leg filled with mercury, and a drop of ether as the other, and its long leg filled to various heights with mercury, according to the temperature. It may be advantageously used from 120° to 140°. For temperatures between 140° and 212°, I have always used a tube similar to the last mentioned, but having its upper extremity sealed, and containing air of common density over the mercurial column, and nearly equal in volume to the capacity of the other leg. When the ether vapour is formed in force, it condenses the said air, and from the condensation, the force is inferred by a well-known law. Having had some reason to suspect my former results by this instrument were somewhat too high; I have been induced to examine the defects to which this instrument is liable. The end of the tube must be drawn out to a point before sealing, and suffered to cool to the temperature of the air; after this, the end must be closed by the point of a flame, otherwise the air in the tube may be rarefied by the heat, in which case the force of the steam will be overrated. Another cause of similar error is the existence of ether vapour in the air at the moment of sealing; this will happen if the tube is not carefully dried inside after the instrument is filled with mercury. In this case, the air in the tube is rarefied by the steam, and consequently is of an unknown but reduced density. The opposite error is liable to be induced, by the frequent use of the instrument. By the motion of the mercury, the small remains of ether mechanically mixed with it rises to the top, and a visible stratum of ether is thereby exposed to the air. In this case an addition of force is given to the air; but as the quantity of this force is known for any temperature, it may be allowed for accordingly. I prefer, however, sealing the tube when well dried, and the air of atmospheric density at the time; and if the ether appear to rise to the surface afterwards, the correction must be applied. In order to have a complete check upon this instrument, it should be adapted so as to be applicable at some temperature (as 140°), where the force is known by other direct means. The error, if any, will thus be shown, and may be calculated for other temperatures.

I have lately made, for the first time, various experiments on the force of steam from water, in temperatures from 212° to 300°; the results which convince me that the theoretic forces which I gave in the fifth volume of the Memoirs, as also those subsequently in my Chemistry, are both erroneous; the former being about as much too small as the latter are too large, so that the mean of the two series is a near approximation to the truth.

Experiments on the force of aqueous steam in high temperatures have been lately made by Mr. Southern, of the Soho, Birmingham,* and by Dr. Ure, of Glasgow,† the results of

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* Dr. Robison's Works by Dr. Brewster.

† It would have given me great pleasure to have been able to adduce Dr. Ure's

which agree very well with each other, and with the mean of my two theoretic tables. As for the force of steam below 212°, no one has found any material variation from those in my first table; indeed scarcely any one seems to have attended much to those below 100°, which I was most anxious to have correct. The force of steam at 32° is an important element; I have spent much time and labour upon it, both before and since my first table was published; it is not less, I think, than 0·2 of an inch, nor more than 0·3; these being the extremes of my experiments; perhaps 0·25 is very near the truth.

My table of the force of alcoholic vapour represents it too high for temperatures below 60°, and for those above rather too low. These errors arose partly from the alcohol not being free from water, and partly from a mistake, as I now apprehend, in fixing a standard mark on the alcohol barometer. They are but small, and of little importance, as the observations were not used in establishing general principles. An improved and more extended series of observations on the force of alcohol vapour has recently been published by Dr. Ure, as mentioned above, the results of which fall in as well as can be desired with those from water, in establishing a general law that the vapours of homogeneous liquids expand in geometrical progression to equal intervals, or at least to *the same* intervals of temperature. I may add, my own experiments recently made for the first time, corroborate those of Dr. Ure in the interval of temperature from 175° to 212°.*

The following skeleton of a table of the force of vapour from water, alcohol, and ether, is formed from what I consider as the most correct experiments hitherto made on these subjects, and may have its use, though it will be found not to differ very materially from my former tables, except where they differ from each other.

experiments on ether also, in corroboration of my early experiments, and of the general principles thence derived; a stronger condemnation of those principles could not have been brought forward than their agreement with the results of Dr. Ure on ether vapour. All the information we have given as to the quality, &c. of his ether is contained in the following paragraph. "The ether of the shops, as prepared by the eminent London apothecaries, boils generally at 112°; but when washed with water or redistilled, it boils at 104° or 105°. It may by rectification, however, be made to boil at a still lower temperature." We are presented with two series of experiments on the force of ether vapour; the first begins at 34° with the force 6·2, and ends at 104°, with the force of 30 inches of mercury; the second begins at 105° with the same force, and ends at 210° with the force of 166 inches. What the specific gravities of the two kinds of ether used were, and whether the ethers used were obtained from the very inferior ether of 112° by washing, or by distillation, are important points, concerning which we are not informed. However, Dr. Ure contrives to blend these two disjointed series, and to compare the results with those of mine made upon ether which boiled at 98°; and finding great discrepances, he concludes my results on ether and principles deduced from them are pregnant with errors.

 * Philosophical Transactions, 1818.

Table of the Forces of Aqueous, Alcoholic, and Ethereal Vapours.

Temperatures (common scale).	Aqueous vapour. Ratio, 2·6.	Alcohol vapour. Ratio, 2·7.	Ethereal vapour. Ratio, 2.
36°	0·29 in.	0·56 in.	7·5 in.
64	0·75	1·51	15·0
96	1·95	4·07 (f)	30·0
132	5·07 (a)	11·00 (g)	60·0
173	13·18 (b)	29·70 (h)	120·0
220	34·20 (c)	80·20 (i)	240·0
272	88·90 (d)	—	—
340 (e)	231·00	—	—

Dr. Ure's numbers for ether corresponding to the above, the last exclusive, are 6·55, 13, 25·7, 49·8 [49], 96·4; the ratio is of course less than two, and a descending one; namely, 1·98, 1·97, 1·94, and 1·93; this last circumstance characterizes a mixed liquid.

I have not extended the experiments on ether further than 212°; but as that temperature gives a force of 207 or 209, I estimate the force to be 240 at 220° nearly.

If the forces registered in the preceding table be allowed as near approximations to the truth, it must, I think, be admitted that they increase in geometrical progression to the same intervals of temperature for a range of 200° at least. Whether those intervals of temperature are equal one to another successively is another inquiry, which the above facts and observations do not enable us to decide.

Analysis of Ether by Electricity, &c.

When a little fluid ether is let up into Volta's eudiometer, either over mercury or water, and a small portion of azotic gas is likewise sent up, in order to be vapourized by the ether; then if the vapourized air be electrified for an hour, some permanent gas is produced, and charcoal is precipitated. The gas when washed is chiefly or wholly carburetted hydrogen; for it takes two volumes of oxygen, and yields one of carbonic acid gas. If the vapourized gas be dry and over mercury, a volume of vapour yields two volumes of carburetted hydrogen, and moisture is perceived within the tube. If the electrification were continued.

(a) Southern, 4·71.   Ure, 4·70
(b) Southern, 13·00.   Ure, 12·95.
(c) Southern, 35·20.   Ure, 35·50.
(d) Southern, 88·00+.   Ure, 89·00.——90.   The mean of my two tables.
(e) This observation is Mr. Southern's. There is reason to suspect his temperatures too high for his forces in the high pressures. They exceed Dr. Ure's.
(f) Ure, 4·02.
(g) Ure, 11·20.
(h) Ure, 30·00.
(i) Ure, 78·50.   Bettan, 82.

no doubt the volume of gas would be greatly increased, and end in pure hydrogen mixed with azote.

These experiments are not decisive ; but they evidently point out the composition of the atom of ether to be 1 carburetted hydrogen, 1 charcoal, and 1 water, or 2 olefiant gas, and 1 water.

The best method of analysis is by firing the vapour of ether mixed with oxygen gas in Volta's eudiometer. This method I discovered in September, 1803, and have used it occasionally ever since. It may be proper to describe the various modifications of which this process is susceptible.

When a few drops of ether are passed through water into the eudiometer containing oxygen gas, the volume of the gas is in a few minutes enlarged more or less, according to the temperature.

In temperatures from 60° to 70°, the volume is about doubled ; but below those it is less than doubled ; and above more than doubled, agreeably to the principle before explained.

(*a*) If the air be doubled or more, and an electric spark be taken in it, the probability is, that no explosion will ensue ; if by repeated sparking an explosion take place, it is feeble, and may be repeated a few seconds afterwards, sometimes once or twice. The residue of gas being examined is found to contain a little carbonic acid, some new combustible gas, and oxygen in various proportions. In short, the operation is very incomplete, owing to an excess of ether vapour.

(*b*) If the oxygen gas be good, and the volume be increased from 100 to 150 by the vapour (which will naturally arise in temperatures between 40° and 50°, and in higher temperatures the volume may be reduced by cautious agitation, till the water has absorbed part of the superfluous ether and vapour), then a spark produces a violent explosion. The gaseous volume is doubled, or from 150 becomes 300 ; and upon examination is found to consist of carbonic acid and new combustible gas, but chiefly the latter. Little or no oxygen is found.

If the ether vapour be only from 3 to 10 per cent. of the volume of oxygen, the explosion is vigorous, and a complete combustion takes place. The residue consists of carbonic acid and oxygen gases only. Ten volumes of ether vapour require about 60 of oxygen, and produce about 40 of carbonic acid.

(*c*) If 100 oxygen be increased by ether vapour to 120 or 130, a violent explosion ensues, and the whole of the vapour is converted into carbonic acid, water, and new combustible gas ; a little charcoal is sometimes deposited, so as to make the air muddy at the instant after explosion ; no oxygen is found in the residue.

(*d*) The combustion of ether vapour may be effected by common air as well as by oxygen gas, only the proportion of vapour to air is very small and limited. If the vapour exceed five per cent. it will not fire ; and if it fall short of two per cent. it rarely fires. The combustion is attended with the production of new *combustible gas,* or otherwise complete, according to the

greater or less proportion of vapour, as is the case with oxygen gas.

In respect to the *new combustible gas* in the above paragraph, its nature may be ascertained by abstracting the carbonic acid in the usual way, and then exploding it with oxygen. In the paragraph (*a*), the new gas is often nearly pure carburetted hydrogen; but in (*c*) and (*d*) it is always a mixture of carbonic oxide and hydrogen in nearly equal volumes; as is proved from its requiring 50 per cent. of oxygen, and producing 50 per cent. of carbonic acid. In (*b*) it is chiefly these two gases, but has a little carburetted hydrogen occasionally mixed with them.

When a certain volume of ether vapour is completely burned at one operation, or it is partially burned at the first, as in (*a*), (*b*), (*c*), and (*d*), and the combustion finished by a second operation, still the same volume of vapour requires the same volume of oxygen for its complete combustion, and produces the same volume of carbonic acid. And it is always found that the carbonic acid contains two-thirds of the oxygen spent, and consequently the hydrogen engages one-third of the oxygen to form water. Hence it appears that the combustible element of ether is olefiant gas; but as there is reason to conclude that oxygen is one of the elements of ether, it must be combined with hydrogen; so that water must be the incombustible element.

In order to find what number of atoms of water and olefiant gas must be combined to form one of ether, we must have regard to the *weights* of the different elements which combine. Now, from the experiments above related, it appears that one measure of ether vapour (weighing 3·1) requires six measures of oxygen gas (weighing 6·6); but two atoms of olefiant gas weigh 12·8, and one of water weighs 8, making together 20·8, which would require six atoms of oxygen, weighing 42, for their combustion; that is, such compound atom would require rather more than double its weight of oxygen, which is the proportion I find by experiment for ether vapour. Hence then we may conclude, that the atom of ether weighs 20·8, and is compounded of one atom of water and two of olefiant gas.

In January, 1809, I made an experiment on the slow combustion of ether in a lamp, in a large balloon glass. The capacity of the balloon was two cubic feet; hence the oxygen of the common air in it would weigh 250 gr. nearly. A small lamp with ether was lighted, and instantly dropped into the balloon, which was immediately closed. The lamp burned till it was extinguished for want of air. After a few minutes it was taken out, and the loss of weight ascertained to be 31 gr. The residuary gas being examined was found to contain 16 per cent. oxygen, and 3 or 4 carbonic acid; but in order to obtain the carbonic acid more accurately, the whole volume of air was *subjected to lime water, in such manner that all the air which came out was agitated in the lime water that entered the balloon. The quantity of lime water requisite to saturate the carbonic*

acid was as much as saturated 107 gr. in weight of dry sulphuric acid = 60 gr. of carbonic acid = 17 charcoal + 43 oxygen. But the oxygen spent in the combustion was $\frac{5}{41}$ of 250 gr. = 60 gr. nearly, of which we find two-thirds, or rather more, in the carbonic acid produced ; the rest must have combined with the hydrogen. And the ether consumed was rather more than one-half of the weight of the oxygen, which may well be supposed to arise from a little loss by evaporation. This experiment, therefore, corroborates the conclusion above obtained.

My first idea of the ether atom, published in the table on the absorption of gases by water in 1803,* was two atoms of carbon and one of hydrogen. This incorrect notion was formed from some of my early experiments combined with the analysis given by others. M. Saussure, in his last essay on ether, has determined its proportions as under; which, being compared with mine, are found to differ from them materially.

	Saussure's.	Mine.
Carbon	67·98	51·9
Oxygen	17·62	33·7
Hydrogen	14·40	14·4
	100·00	100·0

In the present essay I have alluded to the weight of an atom of alcohol; but this weight is not that given in my Chemistry, Part I. From recent experiments on the combustion of alcoholic vapour in oxygen by electricity, as well as from the combustion of alcohol by the platina wire lamp without flame, I believe the alcohol of 0·82 specific gravity is constituted of one atom carburetted hydrogen and one of water, as it seems to give carbonic acid = half the volume of oxygen consumed, or very little more. But there is a remarkable difference in the results when alcohol is burned in a lamp in common air. This combustion gives carbonic acid nearly = two-thirds of the volume of oxygen, and would imply alcohol to be one water and one olefiant gas. At present I have not leisure to clear up this difficulty.

ARTICLE VI.

Calculations of Solar Eclipse to take place on Sept. 7, 1820.
By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR,   Bushey Heath, Jan. 6, 1820.

THE annular eclipse which takes place the 7th of next September will naturally engage the attention of Europe; and it

* Memoirs, vol. i. (Second Series.)

being desirable to have corresponding observations on so rare a phenomenon, I have, with the hope of directing the view of others towards this object, sent you my calculations of the *particular* appearances at this place, as well as the *principal* appearances in other parts of the world. By a mean of several observations of the circumpolar stars, made with an excellent circular instrument, two feet in diameter, and constructed by Mr. Cary, I find the latitude of my Observatory to be 51° 37′ 44·27″ N. and longitude W. in time, 1′ 20·93″ instead of 51° 37′ 42″, and 1′ 20·7″ as shown by Hadley's sextant, and an artificial quicksilver horizon.   I remain, my dear Sir,

<div align="right">Yours very truly,
MARK BEAUFOY.</div>

———

<div align="center">Solar eclipse, Sept. 7, 1820.</div>

Bushey Heath, lat. 51° 37′ 44·27″ N.; long. W. in time, 1′ 20·93″

<div align="right">Apparent Time.</div>

Beginning..........................	0ʰ	21′	50·0″
Visible conjunction.	1	48	23·0
Ecliptic conjunction	1	50	18·6
Greatest obscuration................	1	51	12·0
End	3	12	07·0

Nearest approach of centres, 3′ 02″.   Digits eclipsed on the sun's northern limb, 10° 28′ 08″.   Moon makes the first impression on the sun's disc 54° 48′ from the vertex on the right.

<div align="center">

Places on the Earth where the principal Appearances of the Solar Eclipse occur.

Apparent Time at Greenwich.

</div>

	A. T. at Greenwich.			Latitude.				Longitude.			
Eclipse begins at sun-rise...	11ʰ	22′	07″	A. M.	59°	38′	34″	N	90°	51′	30″ W
Sun rises centrally eclipsed..	12	54	09	P. M.	81	35	42	N	148	50	36 W
Sun centrally eclipsed on the meridian	1	08	09	P. M.	75	50	18	N	17	02	07 W
Sun eclipsed at the true conjunction................	1	51	39	P. M.	55	00	14	N	6	03	36 E
Sun eclipsed at the middle of the transit...............	2	01	34	P. M.	51	18	56	N	8	36	02 E
Sun sets centrally eclipsed..	3	09	00	P. M.	27	01	21	N	45	49	20 E
Eclipse ends at sun-setting..	4	41	02	P. M.	3	15	10	N	20	05	03 E

<div align="center">Duration of the eclipse, 5ʰ 18′ 55″.</div>

A Table containing the Path of the central Eclipse from Sun-rising to Sun-set.

Apparent time at Greenwich, P. M.			Latitude, N.			Longitude from Greenwich.				Apparent time at the place.			
12ʰ	54′	09·0″	81°	35′	49″	148°	50′	36″	W	2ʰ	58′	47′	A. M.
1	01	34·5	80	36	48	32	37	19	W	10	51	03	A. M.
1	11	34·5	73	38	53	12	45	57	W	12	20	31	P. M.
1	21	34·5	68	05	46	5	01	31	W	1	01	28	P. M.
1	31	34·5	63	19	03	0	19	03	W	1	30	18	P. M.
1	41	34·5	59	00	50	3	09	24	E	1	54	12	P. M.
1	51	34·5	55	02	09	6	00	07	E	2	15	35	P. M.
2	01	34·5	51	18	00	8	35	55	E	2	35	58	P. M.
2	11	34·5	47	44	40	11	04	52	E	2	55	54	P. M.
2	21	34·5	44	18	50	13	39	37	E	3	16	13	P. M.
2	31	34·5	40	56	35	16	29	31	E	3	37	32	P. M.
2	41	34·5	37	37	10	19	51	07	E	4	00	59	P. M.
2	51	34·5	34	15	10	24	10	46	E	4	28	17	P. M.
3	01	34·5	30	41	57	30	45	33	E	5	04	37	P. M.
3	09	00·0	27	01	21	45	49	20	E	6	12	17	P. M.

Duration of the central eclipse, 2ʰ 14′ 51″.

By examining Pinkerton's Modern Atlas, the following places are found nearest the moon's tract.

Latitude.			Longitude.			
81°	35′	49″	148°	50′	36″ W	Falls within the polar circle.
80	36	48	32	37	19	Falls also within the polar circle.
73	38	53	12	45	57	Falls over the north-east coast of Greenland.
68	05	46	5	01	31	Falls over the North Sea, nearly due N. of Cape Wrath, N. B.
63	19	03	0	19	03	Falls over ditto, due N. of the Shetland Isles.
59	00	50	3	09	24 E	Falls over ditto, due E. of the Orkney Islands.
55	02	09	6	00	07	Falls over ditto, due E. of Newcastle-on-Tyne.
51	18	00	8	35	55	Falls over Eversberg, a town of the Lower Rhine, in Germany.
47	44	40	11	04	52	Falls nearly over the town of Weilheim, in Bavaria.
44	18	50	13	39	37	Falls over the Adriatic Sea, a little to the S. E. of Pravenna.
40	56	35	16	29	31	Falls nearly over the town of Altamuca in the territory of Barri, in the kingdom of Naples.
37	37	10	19	51	07	Falls over the Ionian Sea, W. of the Gulf of Arcadia, in the Morea.
34	15	10	24	10	46	Falls over the Mediterranean to the S. of Candia.
30	41	54	30	45	33	Falls over a spot nearly W. of the town of Zaera, in L. Egypt.
27	01	21	45	49	20	Falls over the district of Ared, in the Arabia Deserta.

Article VII.

Analyses of Books.

Memoirs of the Literary and Philosophical Society of Manchester,
Second Series. Vol. III. 1819.

This volume of 512 pages contains 24 papers :

I. *Experiments and Observations on Phosphoric Acid, and on the Salts denominated Phosphates.* By John Dalton.—This paper was read to the Society in the beginning of 1813, a circumstance which must not be forgotten while taking a view of its contents. Since that time experiments have been made on the subject by Berzelius, Dulong, Davy, and myself. Of every thing brought to light by these experiments, Mr. Dalton must have been ignorant when his paper was printed, or at least when it was read ; for in a note at the end of his paper (dated Oct. 1817), he notices the labours of Berzelius, Dulong, and myself ; but still adheres to the opinions given in his paper. This being the case, I think it needless to enter into any controversial discussion. I shall, therefore, just give an account of Mr. Dalton's opinions, and, for my own, refer the reader to my review of the Philosophical Transactions for 1818, and to the chapter in my System of Chemistry, in which I give an account of the phosphates.

Mr. Dalton conceives that phosphoric acid might be obtained at a much cheaper rate from the earth of bones than by treating phosphorus with nitric acid. There is no doubt of this. But he has not given any process by which such an extrication can be accomplished. The following would probably answer : Saturate the quadriphosphate of lime, which remains after the earth of bones has been decomposed by sulphuric acid, with ammonia ; filter ; evaporate the solution to dryness ; and expose the dry salt to a red heat. Pure phosphoric acid will remain behind. This process will cost a quantity of sulphuric acid nearly equal to the weight of the earth of bones employed. The quantity of carbonate of ammonia requisite to saturate the acid will be at least equal to double the weight of the earth of bones.

Mr. Dalton finds the composition of earth of bones as follows :'

$$
\begin{array}{lr}
\text{Carbonic acid.} & 3 \\
\text{Lime.} & 11 \\
\text{Phosphate of lime .} & 86 \\
\hline
& 100
\end{array}
$$

He thinks that $8\frac{1}{4}$ parts of concentrated sulphuric acid will be necessary to decompose 10 parts of earth of bones. The acid product obtained is either an octophosphate or dodecaphosphate,

he is not sure which. On the first supposition, it is composed of

$$Acid . . . . 88\tfrac{1}{2}$$
$$Lime . . . . 11\tfrac{1}{2}$$
$$\overline{ 100}$$

On the second,

$$Acid . . . . 92$$
$$Lime . . . . 8$$
$$\overline{ 100}$$

According to the relative weights of the atoms of lime and phosphoric acid, the former of these numbers would indicate a hexaphosphate ; the second an ennea phosphate ; but in none of my experiments, which were very much varied, did I meet with any such compounds.

Mr. Dalton considers the phosphate of lime in the earth of bones as composed of nearly equal weights of acid and base.

He recognised two phosphates of soda, the phosphate and biphosphate, as had been done by chemists long before his time. From the number which he has pitched on to denote the weight of an atom of soda, he distinguishes these salts by the names of biphosphate and quadriphosphate. The following table gives Mr. Dalton's analysis of some other phosphates :

	Acid.	
Phosphate of barytes	100 +	297 barytes.
Phosphate of strontian.	100 +	200 strontian.
Phosphate of magnesia.	100 +	70 magnesia.
Phosphate of alumina	100 +	64 alumina.

Mr. Dalton observes, and thinks the observation new, that nitric and muriatic acid decompose phosphate of lime, as well as sulphuric acid. I will just remind him of one fact. In the original process proposed by Scheele for extracting phosphorus from bones, the first step was to decompose the phosphate of lime by means of nitric acid.

II. *Experiments and Observations on the Combinations of Carbonic Acid and Ammonia.* By John Dalton.—This paper was also read in 1813. He begins it by giving an account of the change which carbonate of ammonia undergoes when exposed to the air. One half of the ammonia gradually flies off, and leaves a bicarbonate of ammonia comparatively fixed, and nearly destitute of smell. I had given an account of this change in the last edition of my System of Chemistry, from my own experiments, which had been made before 1813. I presume the fact was known to other chemists. When I mentioned it to Dr. Wollaston and to Dr. Marcet, I found they were both aware of it. I mention this to show the disadvantage of publishing

chemical papers so many years after they have been read. In 1813, Mr. Dalton, I believe, had he printed his paper, would scarcely have been anticipated in any publication; though I have mentioned three persons who were aware of the facts at least as early.

Mr. Dalton is not disposed to give credit to Gay-Lussac's assertion that one volume carbonic acid gas and two volumes of ammonia condense each other into solid carbonate; but I can give my testimony to the accuracy of this fact, which indeed I have had occasion to repeat more than once. If you mix one volume of ammonia and one volume of carbonic acid gas toge-ther, or two volumes of ammonia and one volume of carbonic acid gas, in either case you get a dry solid salt. The first is a bicarbonate, the second a carbonate; for what Mr. Dalton calls a carbonate, I consider to be a bicarbonate; while his subcar-bonate I consider as a carbonate.

Mr. Dalton conceives that other carbonates of ammonia exist besides these, and has given his reasons for believing in the existence of a subtricarbonate. It may be true that such a salt exists; but his reasons are not sufficient to establish the fact.

III. *Memoirs of the late Charles White, Esq. F.R.S. &c. with Reference to his professional Life and Writings.* By Thomas Henry, F.R.S. &c.—Mr. White was for many years one of the most celebrated surgeons, and possessed one of the most exten-sive practices in the north of England. He was born in Man-chester on Oct. 4, 1728. His father, Dr. Thomas White, was an eminent practitioner of the different branches of medicine, especially of surgery and midwifery. Mr. Charles White was educated in Manchester under the Rev. Mr. Russel, a respect-able clergyman, a good scholar, and a polite and well-bred gentleman. The pupil made a fair progress in classical learning, and at a very early age was taken under his father's professional tuition. In this situation he soon evinced great activity and talent, and began, when almost a boy, to practise in a line which was then generally confided to men of mature age. This early introduction laid the foundation, and, perhaps, was a principal cause of the high character which Mr. White afterwards acquired in that department of the medical profession.

In due time he was sent to attend lectures and hospital prac-tice in London. Here he had for a fellow student Mr. John Hunter, who became afterwards so celebrated as a surgeon and a lecturer, while attending the lectures of Dr. William Hunter. Here they contracted a friendship which lasted for life. During his residence in London, Mr. White devoted his time most dili-gently to professional objects, scarcely allowing himself any time for amusement. He afterwards passed a winter at Edinburgh, at a time when that University was rapidly rising into reputation as a school of medicine.

Having availed himself to the utmost extent of these opportu-

nities of professional improvement, Mr. White joined his father, and soon became an eminent practitioner in his native town. When the Manchester Infirmary was established in 1752, he was appointed the surgeon to that important Institution, and he continued to fill that station till a few years before his death.

During the early part of the last century, the art of midwifery was not so generally practised by males as at present, and the female midwifes were too often extremely ignorant, and were under the dominion of inveterate prejudices. The injurious effects of these deficiencies were more felt in the subsequent treatment of puerperal women than during the time of labour. The lying-in woman was not allowed to rise from her bed before the ninth day; the curtains were drawn around her; the doors and windows were closed; every avenue to the external air was stopped; and a large fire was kept up in the room. She was loaded with blankets, and crammed with caudle, cordials, and broth. The frequent effects of this absurd treatment were puerperal and miliary fevers. Mr. White set himself in opposition to usages so fatal in their consequences, and by great perseverance, a manly spirit, united with great professional ability, and the possession of the public confidence, he was fortunate enough to be ultimately able to accomplish his object. His patients were allowed to rise on the second day; the room was well ventilated, and kept cool; and no cordials or vinous liquors were allowed, except when absolutely necessary, and under proper restrictions. The good effects of these changes were so evident as to carry conviction wherever they were introduced. The miliary fever almost entirely disappeared, and the puerperal soon became comparatively of rare occurrence.

Mr. White was chosen a member of the Royal Society in 1761. On the original institution of the Literary and Philosophical Society of Manchester, he was appointed one of the vice-presidents, an office which he continued almost to the period of his death. In 1803 he was seized with ophthalmia, and suffered long and severe pain in his left eye. The inflammation was subdued, but the sight of the eye was permanently injured. In 1812 the right eye became diseased, a total loss of vision ensued, and his general health rapidly declined. At length, on Feb. 20, 1813, when in the 85th year of his age, he finished a long life of unremitting exertion, and of great and extensive usefulness.

The following is a list of his writings:

In the Phil. Trans. are inserted the following papers: 1. An Account of the Topical Application of the Spunge in the Stoppage of Hæmorrhages; 1772. This practice was afterwards superseded by the invention of the tentaculum, for which surgery is indebted to Mr. Bromfield. 2. An Account of a remarkable Operation on a broken Arm; 1760. 3. An Account of a complete Luxation of the Thigh-Bone of an Adult by external Violence; 1766.

His papers in the Memoirs of the Literary and Philosophical Society of Manchester are the following : 1. On the Regeneration of Animal Substances.. 2. On the Natural History of the Cow so far as is relative to her giving Milk, particularly for the Use of Man; both in vol. i, first series. 3. Observations on a Thigh-Bone of uncommon Length; in vol. ii. 4. An Account of three different Kinds of Trees which are likely to prove a great Acquisition to this Kingdom, both in Point of Profit, and as Trees for Ornament and Shade ; in vol. v.

He published likewise three separate works ; namely, 1. Cases in Surgery. 2. A Treatise on the Management of Pregnant and Lying-in Women. 3. An Essay on the Gradation in Man, and different Animals.

I must here notice a remark of the author of this biographical account that it may not give occasion to too hasty a generalization. " The town of Manchester," he observes, " (the spring water of which contains much calcareous earth) and the surrounding country, afford very few cases of stone ; but it 'is remarkable that those parts of Yorkshire where the water is most free from calcareous impregnation are extremely productive of this terrible disease." It might be inferred from this that pure water had rather a tendency to occasion calculous diseases. But it ought to be attended to that it is very seldom indeed that carbonate of lime or sulphate of lime are found in calculi ; yet these are the calcareous salts which exist in waters. Phosphate of lime, which is the most abundant saline constituent of calculi, exists in bread, and in every kind of animal food. Hence it is rather the food than the water that gives a tendency to this terrible disease. Glasgow was for many years very ill supplied with water. Most of the wells yielded only hard water, and the disease alluded to was fully as uncommon in Glasgow as about Manchester. About 10 years ago two water companies were established in Glasgow, and the city supplied with remarkably pure water (it contains only $\frac{1}{8000}$th part of foreign bodies) from the Clyde ; yet no tendency towards an increase of this disease has been observed.

IV. *Remarks tending to facilitate the Analysis of Spring and River Waters.* By John Dalton.

The object of this paper is to give some rules by which persons but little conversant with chemistry may be enabled to ascertain the goodness of the water with which they are supplied for the purposes of domestic economy or of manufactures. It contains an enumeration of the most common tests, with directions how to use them. The object of the author is laudable ; but I suspect that after all the simplification that has been introduced into the mode of analyzing waters, still, in order to draw the proper inferences from experiment, a good deal of practice and considerable knowledge are requisite. Any body indeed may apply a few tests, and by their means draw a few obvious infer-

ences respecting the foreign substances present in the water under examination. If it becomes milky with lime water, he infers carbonic acid or bicarbonate of lime; if it is precipitated white by oxalate of ammonia, he infers the presence of a salt of lime. Muriate of barytes indicates sulphuric acid, and nitrate of silver muriatic acid. But to determine the quantities of the bodies present in water is much more difficult. I doubt whether Mr. Dalton's methods be susceptible of much precision. It is very difficult to determine the exact point of neutralization in very dilute solutions; nor do I believe that the exact quantities of the liquids employed can be determined by measure. Weight seems the only accurate method. Mr. Dalton expresses his surprise at finding that lime, though supersaturated with carbonic acid, still acts as an alkali on vegetable colours. A little consideration will solve this difficulty. Suppose we dip into a solution of bicarbonate of lime a bit of litmus paper previously reddened by vinegar. The paper owes its red colour to the presence of acetic acid. This acid is capable of displacing carbonic acid from all bases, and uniting with these bases in its place. When the paper is dipped into the liquid, the acetic acid leaves the litmus to unite with the lime; and if a sufficient quantity of lime be present, the paper must resume its blue colour; that is to say, lime must always act as an alkali when it is combined with a weaker acid than that which disguises the vegetable blue colour. There is no difficulty then in seeing the reason of the fact which surprised Mr. Dalton so much.

V. *Account of the Floating Island in Derwent Lake, Keswick.* By Mr. Jonathan Otley.—The name of Floating Island is given to a portion of earth, about six feet in thickness, which rises occasionally in the south-east corner of the lake, not far from Lowdore, generally about 150 yards from the shore. It is attached by one side to the bottom, and sometimes extends to the length of above an acre, and sometimes constitutes only a few perches. Its rise is only at uncertain intervals. Sometimes it appears in two successive seasons, and sometimes not during an interval of seven or eight years. Sometimes it rises a foot above the surface of the lake, and sometimes remains several feet below that surface. It usually rises to the surface at the end of a warm dry season. The bottom of this lake seems to consist of an imperfect kind of peat moss; and Mr. Otley suggests that part of the bottom is occasionally buoyed up to the surface in consequence of the great quantity of gas that is generated within it. Mr. Otley and Mr. Dalton collected a quantity of this gas in 1815. Mr. Dalton found it a mixture of nearly equal volumes of carburetted hydrogen and azotic gases with about six per cent. of carbonic acid gas. No oxygen could be detected in it. This absence of oxygen I think remarkable. I have found common air always mixed with the specimens of carburetted hydrogen gas collected from stagnant water-pools.

(*To be continued.*)

Article VIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Jan. 13, 1820.—Mr. Herschell's paper " On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale of Tints which many of them develope on Exposure to a polarized Ray " was concluded. When Malus published on the present subject, the number of doubly refracting crystals known to philosophers was very limited ; and as the most remarkable of these possessed only one axis of double refraction, it was presumed that Huygen's law applicable to that one might hold good in all. But the discovery of crystals with two axes of double refraction has shown the fallacy of this generalization, and rendered new investigations necessary. The author proceeded to observe that there are two modes of conducting observations on double refraction and polarization ; the one is founded on the immediate observation of the angular deviation of the extraordinary pencil ; the other depends upon the separation of a polarized ray into complementary portions by the action of crystallized laminæ. The author preferred the latter method, and after pointing out its advantages, observed, that to render observations on the tints developed by polarized light available, they must be capable of being compared with one another ; hence the importance of knowing the existence, and tracing the laws of those causes which operate to disturb their regularity. In the author's first inquiries on the polarization of light, he was struck by the great deviation from the succession of colours in thin laminæ, as observed by Newton, that many crystals exhibited when cut into plates perpendicular to one of their axes ; and finding this phenomenon unconnected with irregularities in their thickness or polish, and uniformly repeated in different and perfect specimens, he was led to inquire into their causes, especially as they appeared to form an unanswerable objection to M. Biot's theory, which perfectly explains the tints in crystals with one axis.

In the several sections of this elaborate paper, the author entered into a detailed description of the phenomena, which are reducible to one general fact, viz. that the axes of double refraction differ in their position in the same crystal for the differently coloured rays of the spectrum, being dispersed in one plane over an angle more or less considerable according to the nature of the substance. In many bodies, the magnitude of this dispersion of the axes is comparatively small, while in others not remarkable for a high ordinary or extraordinary dispersive power, it is very great, and renders all computations of the tints in which it is not taken into account completely erroneous. A new element is thus developed, which the author observed must in future enter

into all rigorous formulæ of double refraction; and another striking instance is presented of the inherent distinction between the different coloured molecules of light. At the same time, continued the author, by the complete explanation this principle affords of all the more perplexing anomalies in the tints, the theory of oscillation stands relieved of every difficulty, and may be received as adequate to the representation of all the phenomena of the polarized rays, and entitled to rank with the fits of easy transmission and reflection as a general and simple physical law.

At this meeting a paper, by Dr. Granville, was read, entitled " An Account of a Case of Ovario-gestation." The subject of this case having died suddenly, and under circumstances rather peculiar, an examination of the body was instituted to discover the cause of her decease. On opening the abdomen a quantity of blood was found, and a tumour about four times the size of a hen's egg obstructed the view of the internal parts of generation. This tumour rested on the left portion of the uterus, and upon examination was found to be connected with the left ovarium. At the inferior part of this tumour diaphanous membranes, including the rudiments of a fœtus of about four months' growth, were observed. Upon further examination, it was found that the ovarium where it enveloped the placenta had been ruptured by the growth of the fœtus, and to the loss of blood thus occasioned the death of the mother was attributed. The uterus was considerably developed; the right ovarium was healthy; the left fallopian tube was also sound, and unattached to the tumour. The different stages of the dissection were illustrated by beautiful drawings made by Mr. Bauer.

Jan. 20.—A paper, by Edmund Davy, Esq. was begun, entitled " On some new Combinations of Platinum."

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Sixth Annual Report of the Council.—The state of comparative maturity to which the Society has now arrived affords less interesting matter for remark than during its early progress. The Council, therefore, in discharging this their annual duty to the members, have little left them to do but to call their attention to the respectable rank which the Institution has attained, and to urge the necessity of their continued patronage to insure its stability.

Independently of the intrinsic advantages of an Institution of this kind in gradually adding, by the labours of its members, to the knowledge of the physical structure of Cornwall, it possesses a secondary value by attracting to this part of the county individuals eminent for their genius and scientific acquirements, whose presence cannot fail to be useful to any place which they visit.

Owing to expenses incidental to the completion of a new

museum, the funds of the Society have not, as was expected, as yet justified the addition, by purchase of any new minerals to the cabinet; neither have the donations been so numerous and splendid as last year. The Society has, however, been favoured with not a few specimens, as well from members as others.

The communications on geology and the branches of science connected with it have been numerous and valuable, and the quantity of information contained in several of these respecting the structure of the county and its mineral repositories, renders it the duty of the Council to lay them before the public as soon as materials for a second volume are accumulated, a period probably at no great distance.

The Council regret that the backwardness of many of the members who have it most in their power to forward some of the most interesting objects of the Institution, justifies, and indeed renders necessary, the repetition of the following appeal to their liberality and zeal.

" The Council cannot avoid expressing their regret that so few new specimens have been obtained from the county mines; and that consequently the department of the cabinet set apart for the reception of indigenous ores, which ought to be particularly rich and splendid, continues to be defective, and is eclipsed by many other collections, as well public as private; a circumstance uniformly exciting the surprise of strangers.

"The Council earnestly request the attention of members to the grand object of the Institution; that, namely, of enlarging our knowledge of the geological structure of Cornwall. It is impossible for a few members to undertake the investigation of the whole county. It is, therefore, hoped, that with a view of enabling the Society to complete its long-promised, but still very defective geological map, members will, in their respective districts, endeavour to ascertain the nature and relations of the rocks, and transmit their observations made, and specimens collected, from time to time, to the Secretary, who will be very ready to assist their inquiries by any advice or information in his power. Any person, even although unacquainted with the principles of geological science, can, it is obvious, collect specimens of the various rocks in his vicinity; and members are requested to bear this in mind, with the assurance that collections of this kind, with the various *localities* of the specimens affixed, will very materially promote the important object in view. One grand *desideratum*, and which might be very easily supplied by members resident in the different parts of the county, is to ascertain the exact *limits of the different Granite and Killas districts.* The farmers and miners in any part of Cornwall could give this information to any gentleman that would take the trouble to record it, or to trace the boundary lines in any of the county maps." By order, JOHN FORBES, Sec.

Sept. 21, 1819.

The following papers have been read since the last report:

I. On the Throw of Veins. By Fred. Hall, Esq.—II. On the Importance of Mineralogical and Geological Knowledge to the practical Miner. By J. Forbes, M.D. Sec.—III. On the Granite Veins of Cornwall. By J. Carne, Esq. F.R.S. Hon. M.G.S. Member of the Society.—IV. An Account of the Alluvial Depositions at Sandrycock. By the late P. Rashleigh, Esq.—V. Observations on the Alluvial Strata of Poth, Sandrycock, and Pentuan. By J. Hawkins, Esq. F.R.S. M.G.S. Hon. Member of the Society.—VI. On the Precipitation of Copper. By J. Carne, Esq. F.R.S. &c.—VII. On the Geology of Saint Michael's Mount. By Dr. Forbes.—VIII. On Elvan Courses. By D. Gilbert, Esq. Vice-President of the Royal Society, President. —IX. On the Intersection of Lodes in the Direction of their Dip or Underlie. By J. Hawkins, Esq. F.R.S. &c.—X. On the Geology of the West of Cornwall, Part II. By Dr. Forbes.— XI. Appendix to the above. By Prof. Jameson.—XII. Observations and Experiments on the Construction and Use of a Safety Bar. By J. Ayrton Paris, M.D. F.L.S. Hon. Member of the Society.—XIII. On the different Processes employed in Blasting Rocks; being an Appendix to Dr. Paris's paper. By Dr. Forbes.—XIV. On the Temperature of the Mines of Cornwall. By R. W. Fox, Esq. Member of the Society.—XV. On the Temperature of Mines. By Dr. Forbes.—XVI. Notice on the Geology of the Neighbourhood of Sidmouth. By C. Worthington, Esq.—XVII. On the Origin of the Cornish. By the Rev. S. Greatheed.—XVIII. Notice on the Cornish Minerals in the British Museum. By C. Konig, Esq. F.R.S. Hon. Member of the Society.—XIX. On the Transmission of Heat through different Surfaces. By R. W. Fox, Esq.—XX. Notice on the Coal Field of Pontypool. By W. Llewellin, Esq.—XXI. On an Ebbing and Flowing Spring. By M. Tracelle.—XXII. Notice on the Employment of a Mixture of Sawdust and Gunpowder in Blasting Rocks. By Sir C. Hawkins, Bart. M.P. F.R.S. a Vice-President of the Society.—XXIII. Notice of the Quantity of Tin and Copper raised in Cornwall; and of the Quantity of Copper raised in Great Britain and Ireland in the Year ending June 30, 1819. By Joseph Carne, Esq. F.R.S. &c.

At the Anniversary Meeting, Sept. 21, 1819, D. Gilbert, Esq. M.P. V.P.R.S. Pres. in the Chair, the Report of the Council being read, it was resolved,

That it be printed and circulated among the members.

That the thanks of the Society be presented,

1. To the authors of the various papers read; and the donors of specimens, books, &c.

2. To the officers of the Society.

That the papers of Dr. Paris and Dr. Forbes on the subject of blasting rocks be printed and circulated among the county mines.

Comparative View of the Number of Members.—Last anniversary, 172; removed and dead, 12; elected this year, 4; total, 164.

Officers and Council for the present Year.

President.—D. Gilbert, Esq. M.P. V.P.R.S. &c. &c.
Vice-Presidents.—Sir Rose Price, Bart.; Sir W. Lemon, Bart.; L. C. Daubuz, Esq.; Rev. G. Treweeke.
Secretary.—John Forbes, M.D.
Treasurer.—Henry Boase, Esq.
Librarian.—Rev. C. V. Le Grice, A.M.
Curator.—Edward C. Giddy, Esq.
Assistant-Secretary.—R. Moyle, Jun. Esq.
The Council.—J. Carne, Esq.; T. Hartley, Esq.; M. P. Moyle, Esq.; J. Rule, Esq.; J. Tremenheere, Esq.; Rev. Uriah Tonkin; J. Giddy, Esq.; W. Sandys, Esq.; G. D. John, Esq.; J. Stevens, Esq.

Article IX.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Analysis of a Specimen of Blende.* By Du Menil, Apothecary at Wunstorf.

The colour of the mineral was reddish-brown. Its fracture was foliated. Its specific gravity was 4·061. Its powder was light-brown. The analysis conducted in the same manner as mine, described in a preceding volume of the *Annals of Philosophy*, gave the following constituents:

Sulphur.	11·58	23·16
Zinc	34·24	68·48
Iron	4·04	8·08
	49·86	99·72
Loss	0·14	0·28
	50·00	100·00

(Schweigger's Journal, xxiv. 67.)

II. *Chemical Analysis of Egeran.* By Stanislaus Count Dunin Borkowski.

The mineral called Egeran by Werner, and considered by him as a species distinct from idocrase, is found at Haslau, near Eger, in Bohemia, and has for some years been well known to mineralogists. Haüy considers it as a variety of idocrase; and as far as can be judged from the cleavage, this opinion seems to

be so far correct that the primitive form of the crystals is the same in both. At the same time there is a considerable difference in the appearance of the two minerals, and likewise in the chemical composition, though this last may be owing to foreign matter with which the Egeran is always contaminated. Its specific gravity is 3·294. The following table exhibits the constituents of Egeran, as determined by the analysis of Count Borkowski :

Silica....................... 41
Alumina..................... 22
Lime 22
Magnesia 3
Iron........................ 6
Manganese.................. 2
Potash..................... 1
                           ——
                           97

(Schweigger's Journal, xxiii. 387.)

Idocrase, according to the analysis of Klaproth, is composed as follows :

Silica..................... 35·50
Alumina................... 22·25
Lime...................... 33·00
Oxide of iron .... 7·50
Oxide of manganese. ...... 0·25
                           ——
                           98·50

So that Egeran contains less silica and more lime than idocrase. But if a mineral from Siberia, which Klaproth considered as an idocrase, was really one, as there is every reason, from his description of it, to conclude, then the composition of idocrase and Egeran is more nearly the same. Klaproth found the composition of the Siberian mineral as follows :

Silica..................... 42·00
Alumina................... 16·25
Lime...................... 34·00
Oxide of iron. .... 5·50
Oxide of manganese. ...... Trace
                           ——
                           97·75

The specific gravity of the Siberian mineral was 3·365—3·390. Its colour was dark olive-green. The crystals were rectangular four-sided prisms, with the edges sometimes so much truncated that they assumed the appearance of eight-sided prisms.

III. *New Nickel Ore.*

Cronstedt made known a new nickel ore found at Helsing, in

Sweden; but did not describe it. Prof. Pfaff, of Kiel, has lately published a description and analysis of it. Of these I shall give an abstract in this place.

The colour of the mineral, when recently broken, is light lead-grey, approaching to tin-white; but it gradually tarnishes, and approaches the appearance of copper nickel.

It occurs massive.

The lustre of the fracture is splendid and vitreous.

The fracture is foliated.

Composed of granular distinct concretions, resembling steel-grained galena.

Fragments indeterminate and blunt-edged.

Opaque; streak similar.

Semihard; very easily frangible; specific gravity, 6·129. Its constituents are as follows:

Nickel. .... 24·42
Arsenic .... 45·90
Iron .... 10·46
Sulphur. .... 12·36
                          ———
                          93·14

(Schweigger's Journal, xxii. 253.)

IV. *Position of Lyons.*

In the year 1811, Baron von Zach determined the latitude of this city by means of 262 observations to be 45° 45′ 57·37″. Gabriel Mouton, a celebrated astronomer of Lyons, during the 17th century, determined the latitude to be 45° 45′ 35·1″. But the position of his Observatory is not accurately known. The longitude is 10′ in time E. from Paris, or 22° 29′ 9″ E. of Ferro.
—(Correspondence Astronomique du Baron de Zach, i. 205.)

V. *Clock.*

The first clock ever known in France was erected in the fifth century, in the Cathedral church of Lyons. Gondebaut, or Gombaut III. King of Burgundy, having been informed that Theoderic, King of the Goths, who resided at that time in Ravenna, had machines which marked the order of time, according to the movements of the heaven and stars, wrote to him requesting to have one. Theoderic gave orders to the celebrated Boccius to make for him two such as perfect as possible. Theoderic sent them to Gondebaut, with an excellent letter which may be seen in the works of Cassiodorus, Secretary of State to Theoderic, who was accustomed towards the end of his days, after he had retired from public affairs, to amuse himself with making quadrants, clocks, and perpetual lamps.—
(Ibid. p. 225.)

VI. *Declination of the Needle at Lyons.*

1751, in November. .... 15° 45′ W
1752 . ... 16　00
1755, in December .... 16　30
1757, in December .... 17　15
1760 . ... 18　30
1761 . ... 18　45

Thus the increase amounts to 9′ or 10′ in the year.—(Ibid. p. 223.)

VII. *Position of Lucca.*

Father Inghirami, during a residence at Lucca in 1818, had an opportunity of making a set of observations to determine the latitude and longitude of this celebrated city.　The following are the results :

Latitude . ... 43° 50′ 49·5″
Longitude .... 28　10　25·9 E. of Ferro.

In 1809, Baron von Zach determined the difference between the longitudes of Pisa and Lucca by means of a chronometer. The result gave him for the longitude of Lucca,

28° 10′ 25·8″

a result which almost coincides with the observations of Inghirami.

The latitude of Lucca, determined by Baron von Zach's observations, is,

43° 50′ 50·54″.

Numbers which likewise correspond well with the determination of Inghirami.—(Ibid. p. 227.)

VIII. *Position of Montpellier.*

The latitude of Montpellier was ascertained in 1811, by Baron von Zach, and found to be

43° 36′ 15·71″

The longitude is 6′ 10″ in time E. from Paris.—(Ibid. p. 247.)

IX. *Determination of some Places on the Coast of Sicily.*
By Mr. Charles Rumker.

	Latitude.			Longitude.		
Fiumicino.	41°	45′	00″	29° 52′	00″	E. of Ferro
Isle of Ustica.	38	43	01	30 52	0	
Isle of Maritimo.	38	1	00	29 43	45	
Isle of Favignana......	37	57	00	29 55	9	
Girgenti	37	15	52	31 11	21	

(Ibid. p. 249.)

X. *Occultations of Stars behind the Moon, observed at the Obser-*
vatory of the Royal Academy of Turin between 1812 *and* 1817,
with an Achromatic Telescope, of Dolland, of 42 *Inches Focus,*
and 44 *Lines of Opening.* By M. Plana, *Astronomer Royal.*

I am induced to copy the following table on account of the
great utility that it may be of to navigators, &c. in determining
the longitude of places. The longitude of Turin being accurately
known, and the observations of M. Plana being celebrated for
their accuracy.

Date.	Star eclipsed.	Mean solar time.	Circumstances.
1812.			
Oct. 21	ƒ of Taurus.	10ʰ 7′ 58·8″ imm.	
22	6ᵃ of Taurus.	8 42 47·2 imm.	
22	Aldebaran.	12 4 32·5 imm.	
1813.			
March 6	μ Balænæ.	9 26 50·0 imm.	
8	Aldebaran.	{ 7 2 5·8 imm.	
	....	{ 8 13 14·5 emer.	
April 17	γ Libræ.	{ 10 56 49·2 imm.	
		{ 12 16 6·6 emer.	A little doubtful.
Nov. 7	μ Balænæ.	12 56 7·2 imm.	
Dec. 28	ψ Aquarii.	{ 8 7 50·6 imm.	
		{ 9 14 35·3 emer.	Doubtful to about 3″
1814.			
Jan. 1	μ Balænæ.	9 51 15·9 imm.	
Nov. 25	μ Balænæ.	4 57 50·3 imm.	
1815.			
March 19	δ Gemini.	{ 11 44 20·7 imm.	
		{ 12 29 8·8 imer.	Doubtful to 1″ or 2″.
1816.			
Feb. 19	β Scorpion.	{ 15 20 40·4 imm.	
		{ 16 22 8·3 emer.	Doubtful.
Oct. 4	30 Piscis.	10 15 2·4 imm.	
Nov. 12	η Leonis.	{ 14 18 54·1 imm.	Rather doubtful.
		{ 15 28 18·7 emer.	
1817.			
Feb. 2	η Leonis.	10 30 27·1 imm.	
8	x Libræ.	{ 16 16 56·1 imm.	
		{ 17 28 28·9 emer.	
March 29	η Leonis.	{ 7 22 49·9 imm.	
		{ 8 34 55·5 emer.	1″ of uncertainty.

(Ibid. p. 272.)

XI. *Position of Places on the Coast of the Adriatic Sea, deter-*
mined by Capt. Smyth.

	Latitude.	Longitude E.	
		From Greenwich.	From Ferro.
Isle of Fano (western point)	39° 50′ 20″	19° 19′ 50″	36° 59′ 36″
Isle of Merlere	39 52 50	19 35 10	37 14 55
Capo Drasti.	39 47 10	19 42 0	37 21 45
Fort Alessandro at Vido	39 38 5	19 55 38	37 35 23
Capo Bianco	39 20 50	20 6 50	37 45 35

The position of the coasts of the Adriatic Sea not being yet well determined, Baron von Zach has added a few more facts to those above stated by Capt. Smyth. It will be worth while to mention some of the principal of these.

1. Lecce, a town between Otranto and Brindisi, the Aletium of the ancients, was visited by Rizzi-Zannoni in 1786, who, being provided with an excellent Ramsden's quadrant of two feet radius, determined its position as follows :

<div align="center">Latitude, 40° 41′ 4·2″.</div>

2. Baron von Zach, being at Venice in 1807, determined the position of a great number of places in that city. It will be sufficient to give one of these here; namely, the steeple of St. Mark.

Latitude .... 45° 25′ 58·1″
Longitude 30   0   37·2 E. from Ferro.

3. Being at Rimini in 1808, a city belonging to the states of the church, Baron von Zach determined the position of the following places :

	Latitude.	Longitude from Ferro.
Rimini le fanal...................	44° 4′ ·37″	30° 12′ 18″·
Rimini palais Garampi.............	44 3 46	30 12 36
Tour de la Fontanella.	43 59 21	30 18 37
Convent sur le mont Laro.	43 54 43	30 24 20 ,

4. In 1799 the Austrian government sent the astronomer M. Bogdanich into Hungary to determine the geographical position of the principal places. Among the rest he determined the position of the two following places on the coast of the Adriatic.

	Latitude.	Longitude.
Fiumi (garden of the Cordeliers). ..	45° 20′ 10″	32° 5′ 20″·
Carlopago (garden of the Comm.)..	44 31 35	32 54 40 ·

5. M. Quenot on his return from the unfortunate expedition to Egypt about the end of 1798, determined the position of the two following places :

	Latitude.	Longitude.
Ragusa.........	42° 36′ 30″	...... 35° 51′ 40″·
Lissa............	43 10 00	...... 33 51 00

6. The brother of Baron von Zach, at that time in the Austrian service, was employed between 1801 and 1805 in making a survey of the *Duchy of* Venice. He determined the position of the following places on the coast of the Adriatic :

	Latitude.			Longitude.		
Aquileja	45°	45′	54″	31°	2′	4″
Bocca de Tiel.	45	41	0	31	9	4
Caorle (Porto di Falconera)	45	35	42	30	33	49
Capo d'Istria (Istria)	45	30	36	31	22	33
Cavolino (Porto di Reve-vecchia) . . .	45	28	51	30	13	27
Duino (Porto dei Bagni)	45	46	50	31	3	43
Fiume . . . . :	45	20	12	32	5	30
Grado (Porto Grado)	45	40	17	31	3	36
Marano	45	29	28	29	40	54
Nardo cortello.	45	40	9	30	44	32
S. Georgio di Pirano (Istria).	45	29	44	31	14	19
Trieste.	45	38	30	31	26	53

7. Boscovick and Maire, while employed in laying down a map of the states of the church, determined the position of the following places :

	Latitude.				Longitude.		
Ancona	43°	37′	54″	. . . . . .	31°	9′	45″
Cervia.	44	15	31	. . . . . .	30	0	21
Comacchio.	44	40	27	. . . . . .	29	50	40
Fano	43	51	0	. . . . . .	30	40	31
Fermo.	43	10	18	:	31	22	19
Loreto	43	27	0	. . . . . .	31	15	43
Pesaro.	43	55	1	. . . . . .	30	34	14
Ravenna	44	25	5	. . . . . .	29	31	29
Rimini.	44	3	43	:	30	13	29
Ripa transone.	43	0	24	. . . . . .	31	25	23
Sinigaglia	43	43	16	. . . . . .	30	52	23

(Ibid. p. 274.)

XII. *Isle of Elba.*

It has been commonly believed that the Isle of Elba, in consequence of the inexhaustible mines of iron in which it abounds, especially Mount Calamita, which is supposed to be a solid mass of loadstone, has a sensible effect upon the needle. Hence it has been thought that vessels in the neighbourhood of this island could no longer depend upon the declination of their needles, being the same as at a distance. This was tried by Mr. Charles Rumker in 1818, but at the distance of two, three, or four nautical miles from the island he did not find that the needle in his vessel was in the least affected by the action of the island. The variation of the compass was 18° 18′ 40″ W.— (Ibid.)

XIII. *Population of the Calton in Nov. 1819.*

The Calton is a suburb of Glasgow inhabited chiefly by journeymen manufacturers. It has been lately erected into a burgh of barony, and in consequence, the following enumeration of the

inhabitants and of their professions was taken. It is a document of so curious a nature that I think it well worth preserving, by inserting it into a journal more likely to be preserved than a common newspaper.

Female householders.	1958
Labourers and porters	1097
Weavers.	5132
Wrights and sawyers.	601
Shoemakers.	721
Taylors.	253
Cotton spinners	839
Manufacturers.	120
Merchants (shopkeepers).	101
Warpers and starchers	283
Nailers and toolmakers.	125
Blacksmiths and tinsmiths.	233
Booksellers.	57
Dyers and hatters	96
Warehousemen	153
Bricklayers and plasterers.	178
Masons and slaters.	229
Butchers.	68
Preachers and teachers.	64
Clerks and surveyors.	119
Barbers and hecklers.	52
Engravers and founders.	100
Printers and turners	76
Causewayers	38
Cloth cappers.	46
Letter carriers	18
Messengers and sheriff officers.	15
Hosiers	105
Watchmakers and china dealers	23
Brush and umbrella makers.	33
Saddlers and tanners.	20
Brokers and pedlars	70
Coopers and gardeners.	49
Pensioners and sailors	101
Watchmen and chapmen	71
Cowfeeders.	45
Glass and pin makers, engineers	51
Brewers and bakers	217
Carters and horse dealers.	209
Portioners and surgeons.	28
Lodgers and servants.	1309
Grocers and spirit dealers.	512
	15,616

Nations.

Scotch	12,222
Irish.	3,212
English	162
Americans	8
Germans	3
Swedes.	3
Italians.	6
	15,616

XIV. *Meteorological Observations at Cork.* By T. Holt, Esq.
(With a Plate. See CI.)

(To Dr. Thomson.)

DEAR SIR,   *Cork, Oct.* 9, 1819.

I ᴇɴᴄʟᴏsᴇ you the report of my meteorological observations made in and near Cork, for the third quarter of 1819; and am, dear Sir, with due respect,

Your obedient humble servant,

Tʜᴏᴍᴀs Hᴏʟᴛ.

REMARKS.

JULY.

1. Cloudy ; dry.
2. Rainy morn and evening.
3. Cloudy.
4, 5, 6. Bright days.
7. Bright; breeze.
8. Dry ; clouded.
9, 10, 11, 12. Bright days.
13. Dry; overcast.
14. Rainy morning ; cloudy.
15. Cloudy; showers.
16. Dry, dull day.
17. Bright.
18. Rainy ; cloudy.
19. Rainy ; showery day.
20, 21, 22, 23, 24. Bright days.
25. Cloudy; showers.
26. Dry ; cloudy.
27, 28, 29, 30, 31. Bright days.

AUGUST.

1. Bright ; thunder.
2. Ditto ; lightning and thunder, with rain.
3. Overcast; dry.
4. Bright.
5, 6. Cloudy; light showers.
7. Cloudy; dry.
8. Heavy showers.
9. Cloudy; dry.
10. Ditto ; rainy evening.
11. Cloudy; dry.
12. Rainy.

13. Overcast; dry.
14. Ditto, ditto.
15. Bright.
16, 17, 18. Bright days; fog morning and evening.
19, 20, 21, 22, 23, 24. Bright days.
25, 26. Clouded, dry days.
27. Bright.
28, 29. Heavy showers.
30. Ditto, ditto, windy.
31. Dry; windy.

SEPTEMBER.

1. Bright.
2. Cloudy ; rainy evening.
3. Bright.
4. Rainy morning and evening.
5. Bright ; occasional showers.
6. Cloudy.
7. Bright.
8. Cloudy ; light showers.
9. Rainy morn ; light showers.
10, 11, 12, 13, 14, 15, 16. Bright days.
17. Cloudy.
18. Bright.
19. Overcast.
20, 21, 22, 23. Bright days.
24, 25. Light showers.
26, 27. Fair ; rainy evenings.
28. Bright ; occasional showers.
29. Bright ; windy.
30. Rainy ; windy.

July 1819   August   September
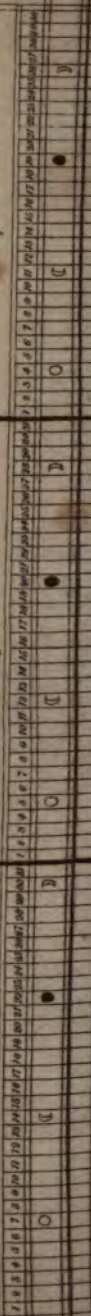
Wind.
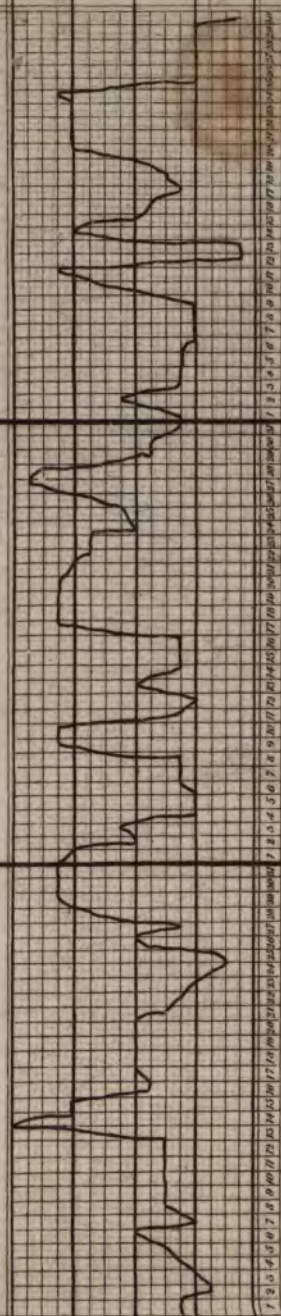
RAIN.

1819.	Inches.	1819.	Inches.	1819.	Inches.
July 2	0·390	Aug. 2	0·398	Sept. 2	0·312
8	0·294	5	0·058	4	0·521
11	0·035	6	0·140	5	0·228
12	0·016	8	0·180	8	0·132
14	0·636	10	0·156	9	0·384
15	0·125	12	0·096	24	0·180
18	0·785	28	0·364	25	0·045
19	0·050	29	0·066	26	0·054
25	0·160	30	0·968	27	0·174
		31	0·145	28	0·120
	2·491			30	0·245
			2·571		
					2·395
					2·571
					2·491
					7·457

XV. *New Rockets.*

Capt. Schumacher, brother of Professor Schumacher, Astronomer Royal at Copenhagen, has invented a new kind of rocket, which is said greatly to surpass the Congreve rockets, both in their force and in the accuracy with which they may be thrown. The King of Denmark has established a new corps of artillery (*Baketer-corps*) commanded by Capt. Schumacher, whose business is to throw these rockets. They ascend to a very great height in the air; and when they have reached the highest point, a globe of fire makes its appearance, which is so vivid that it may be seen at the distance of 70 miles. Capt. Schumacher elevated them in the island of Hjelm in 1816, and they were seen distinctly by his brother at Copenhagen at the distance of 17¼ German miles.—(Correspondence Astronomique du Baron de Zach, i. p. 266.)

XVI. *Pink Sediment of Urine.* By Dr. Prout.

I lately had an opportunity of examining the most marked specimen of pink sediment I had ever seen. It consisted almost entirely of the lithate of ammonia. Pink sediments in general consist either of this substance, or of the lithate of soda mixed with more or less of the phosphates. When the lithate of soda and the phosphates prevail, the sediment usually assumes the form of what has been denominated the lateritious sediment. In specimens of this latter description, I have several times found nitric acid; and in all cases I have satisfied myself that the red colour of these sediments depends upon some slight mixture of the purpurate of ammonia, or of soda, according as the sediment itself consists of the lithate of ammonia or soda. Perhaps the formation of the purpuric acid may be explained by supposing that the nitric and lithic acids are secreted together, and that the purpuric acid, or rather the purpurate of ammonia, is formed by the action of the nitric upon the lithic acid.

ARTICLE X.

Magnetical and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1819. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Dec. 1	8ʰ 35′	24° 32′ 52″	1ʰ 15′	24° 39′ 19″		
2	8 35	24 32 10	1 50	24 38 48		
3	8 35	24 32 40	1 20	24 36 55		
4	— —	— — —	1 10	24 37 47		
5	8 45	24 34 31	1 25	24 36 39		
6	8 35	24 32 34	1 20	24 36 58		
7	8 40	24 32 49	1 20	24 37 35		
8	8 40	24 33 57	1 25	24 38 29		
9	8 45	24 35 03	1 20	24 39 30		
10	8 40	24 35 04	1 20	24 37 02		
11	8 45	24 32 36	1 20	24 39 43		
12	8 40	24 36 16	1 20	24 37 36		
13	8 45	24 34 44	1 20	24 37 07		
14	8 45	24 32 05	1 25	24 36 19		
15	8 45	24 31 57	1 20	24 38 20		
16	8 40	24 32 00	1 20	24 37 05		
17	— —	— — —	1 20	24 39 18		
18	8 40	24 33 26	1 25	24 36 09		
19	8 45	24 32 26	1 20	24 35 56		
20	8 50	24 34 12	1 25	24 35 11		
21	8 45	24 33 56	— —	— — —		
22	8 40	24 34 42	1 20	24 37 14		
23	8 45	24 31 17	1 20	24 36 02		
24	8 40	24 33 28	1 15	24 37 02		
25	8 45	24 32 28	1 45	24 36 07		
26	8 40	24 34 25	1 30	24 37 16		
27	8 45	24 36 33	1 20	24 38 20		
28	8 45	24 34 14	1 20	24 37 32		
29	8 45	24 31 36	1 20	24 36 11		
30	8 45	24 33 11	1 30	24 37 37		
31	8 45	24 33 35	1 25	24 36 25		
Mean for Month.	8 42	24 33 29	1 23	24 37 23		

Owing to the shortness of the days, evening observations discontinued.

Rain, by the pluviameter, between noon the 1st of December, and noon the 1st of January, 2·429 inches. Evaporation, during the same period, 0·71 inch.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Dec.		Inches.				Feet.		
	Morn....	29·277	45°	87°	WNW		Showery	45°
1	Noon....	29·456	47	73	WNW		Very fine	48
	Even	—	—	—	—			35½
	Morn....	29·513	45	85	SSW		Rain	
2	Noon....	29·453	47	89	SW		Rain, fog	48
	Even	—	—	—	—			32
	Morn....	29·696	33	86	S by W		Clear	
3	Noon....	29·690	39	75	SSW		Very fine	42
	Even	—	—	—	—			36½
	Morn....	29·225	—	92	SSW		Hardrain	
4	Noon....	29·105	47	71	W by N		Cloudy	47
	Even	—	—	—	—			38
	Morn....	29·400	33	83	NE		Rain	
5	Noon....	29·443	38	80	ENE		Cloudy	39
	Even	—	—	—	—			34
	Morn....	29·560	35	78	ENE		Cloudy	
6	Noon....	29·578	34	80	ENE		Sleet	35
	Even	—	—	—	—			32
	Morn....	29·558	32	83	NE		Snow	
7	Noon....	29·516	32	83	NE		Sleet	32½
	Even	—	—	—	—			27½
	Morn....	29·516	28	76	E		Cloudy	
8	Noon....	29·545	27	68	E		Very fine	27
	Even	—	—	—	—			19
	Morn....	29·562	20	81	NE		Very fine	
9	Noon....	29·550	27	76	ENE		Cloudy	27
	Even	—	—	—	—			25
	Morn....	29·400	26	83	NNE		Snow	
10	Noon....	29·424	29	78	NNW		Snow	29
	Even	—	—	—	—			18
	Morn....	29·517	20	78	W by N		Clear	
11	Noon....	29·510	26	65	W by S		Clear	27
	Even	—	—	—	—			21
	Morn....	29·451	27	81	WSW		Fine	
12	Noon....	29·405	33	70	W		Cloudy	33¼
	Even	—	—	—	—			23
	Morn....	29·300	25	72	WNW		Clear	
13	Noon....	29·283	30	70	WNW		Cloudy	33
	Even	—	—	—	—			22
	Morn....	29·240	23	83	SW by S		Fine	
14	Noon....	29·153	28	78	SW by S		Very fine	33⅔
	Even	—	—	—	—			—
	Morn....	28·955	30	83	WSW		Cloudy	
15	Noon....	28·956	35	73	WSW		Cloudy	36
	Even	—	—	—	—			29
	Morn....	29·355	32	79	W		Very fine	
16	Noon....	29·454	36	71	W by N		Very fine	37⅔
	Even	—	—	—	—			31
	Morn....	29·193	—	93	SSE		Stormy	
17	Noon....	28·985	40	91	S		Rain	50½
	Even	—	—	—	—			35⅔
	Morn....	28·903	50	92	SW		Fog, rain	
18	Noon....	28·986	52	85	WSW		Cloudy	52
	Even	—	—	—	—			49
	Morn....	29·376	50	94	SW		Fog	
19	Noon....	29·350	51	93	SW		Rain	53
	Even	—	—	—	—			

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Dec.		Inches.				Feet.		
	Morn....	29·190	51°	95°	SW		Rain	55°
20	Noon....	29·183	54	83	W		Showery	54
	Even....	—	—	—	—		—	44
	Morn....	29·484	44	80	NNW		Cloudy	44
21	Noon....	29·418	—	83	SE		Rain	54
	Even	—	—	—	—		—	41
	Morn....	29·216	50	90	W by S		Rain	
22	Noon....	29·243	51	73	W by N		Cloudy	52½
	Even	—	—	—	—		—	41
	Morn....	28·823	41	82	NW by W		Cloudy	
23	Noon....	28·838	41	69	NW by W		Fine	44
	Even ...	—	—	—	—		—	30
	Morn....	28·833	30	84	WSW		Very fine	
24	Noon....	28·860	35	72	Var.		Very fine	36⅔
	Even	—	—	—	—		—	27⅓
	Morn....	28·865	31	80	NW by N		Cloudy	
25	Noon....	28·905	32	73	NW by N		Very fine	32⅔
	Even	—	—	—	—		—	23
	Morn....	29·000	24	82	W		Very fine	
26	Noon....	29·008	30	75	S		Clear	30
	Even....	—	—	—	—		—	26⅔
	Morn....	28·770	28	79	ESE		Fine	
27	Noon....	28·964	31	73	ESE		Fine	31½
	Even....	—	—	—	—		—	25
	Morn....	28·996	31	78	NE		Cloudy	
28	Noon....	28·992	31	74	ENE		Snow	31½
	Even	—	—	—	—		—	24
	Morn....	29·130	26	84	N		Cloudy	
29	Noon....	29·158	30	77	NNW		Cloudy	30
	Even	—	—	—	—		—	19⅔
	Morn....	29·039	21	80	WSW		Fine	
30	Noon....	28·974	29	75	S		Cloudy	29½
	Even	—	—	—	—		—	24
	Morn....	28·868	25	77	SSE		Very fine	
31	Noon....	28·863	31	67	SSW		Very fine	31
	Even	—	—	—	—		—	

ANNUAL RAIN TABLE.

Month.	Rain.	Evaporation.
January.	1·906	1·400
February	2·828	1·490
March	1·152	2·680
April............	2·468	3·440
May.	3·068	4·530
June	1·950	4·250
July............	1·514	4·990
August	2·520	4·790
September	3·213	3·550
October.	1·610	2·280
November.	1·761	1·230
December.	2·429	0·710
Total Mean......	26·415	35·150

As the evaporation for the year exceeds the quantity of rain, it may be advisable to describe the evaporating apparatus. It consists of a hollow cylinder, one foot in diameter, elevated to the same height as the rain-guage, and sheltered by a copper roof, placed some inches above the edge of the bason. The eves of the roof project, which permits a free current of air, but excludes the rain.

ARTICLE XI.

METEOROLOGICAL TABLE.

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1819.	Wind.	BAROMETER. Max.	Min.	THERMOMETER. Max.	Min.	Evap.	Rain.	Hygr. at 9 a. m.	
12th Mo.									
Dec. 1	N W	30·08	29·81	49	31			99	☉
2	S W	30·23	30 02	50	25		—	88	
3	S W	30·23	29·81	44	25		—	100	
4	S W	29·96	29·70	48	38		60	100	
5	N E	30·10	29·96	41	34			79	
6	N E	30·15	30·10	37	33		—	81	
7	N E	30·15	30·14	35	28			89	
8	E	30·19	30·15	30	18			73	
9	N E	30·15	30·04	30	23		—	68	☽
10	N	30·15	30·04	33	10			87	
11	S W	30·14	30·11	31	13			84	
12	S W	30 11	29·95	38	13		—	75	
13	S W	29·95	29·88	36	20		—	88	
14	S W	29·88	29·61	37	23		—	92	
15	W	29·97	29·58	38	29			80	
16	N W	30·10	29·81	41	27		41	85	
17	S E	29·81	29·41	53	38		44	98	
18	S W	29·89	29·41	55	50		—	100	●
19	S W	29·89	29·76	54	52		15	99	
20	S W	30·00	29·74	56	44			100	
21	Var.	30·00	29·79	52	41	56	49	84	
22	S W	29·79	29·30	54	48		31	95	
23	N W	29·39	29·30	49	29			91	☾
24	S W	29·39	29·35	36	24			96	
25	N W	29·58	29·39	34	21			85	
26	S W	29·58	29·55	32	23			81	
27	S E	29·59	29·55	34	27			85	
28	N E	29·73	29·59	35	23		—	89	
29	N	29·73	29·66	32	19			96	
30	S W	29·66	29·51	32	17			92	
31	SE SW	29·51	29·40	33	11	23	05	86	
		30·23	29·30	56	10	0·79	2·45	68—100	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—1. Cloudy. 2. Rainy morning. 3. Hoar frost: fine day. 4. Rainy morning and evening. 5, 6. Overcast. 7. Some snow this morning: much wind at night. 8. Fine: windy: thermometer at Tottenham, 16°. 9. Snow in the night, covering the ground above an inch deep. 10. *Cirrus, Cirrocumulus*: fine: thermometer at Tottenham, 8°. 11. Very foggy morning: gloomy day. 12. Morning, fine: snow and sleet, p. m. 13. A little sleet: about six, p. m. a shower of rain, after which it began to freeze again. 14. *Cirrocumulus*: fine. 15. Foggy morning: a thaw commenced about 10, a. m. and went on till about the same hour, p. m. 16. Foggy: frost continued through the day, but at night it was very stormy, with rain from the S and SE. 17. Rainy. 18—20. Cloudy and gloomy. 21. Rain. 23. *Cirrus, Cirrostratus, Cumulostratus.* 24—27. Hoar frosts: fair. 28. A pretty considerable fall of snow, leaving an inch depth on the ground after, in part, melting as it fell. 29—31. Fine: *Cirrus*: snow on the ground.

RESULTS.

Winds: N, 2; NE, 5; E, 1; SE, 3; SW, 14; W, 1; NW, 4; Var. 1.

Barometer: Mean height

        For the month. .... 29·814 inches.
        For the lunar period, ending the 8th. .... 29·875
        For 14 days, ending the 10th (moon north) ...... 29·989
        For 13 days, ending the 23d (moon south) .... 29·790

Thermometer: Mean height

        For the month.................................... 34·12°
        For the lunar period (as above) .... 37·88
        For 30 days, the sun in Sagittarius................ 36·65

Hygrometer: Mean for the month . ... 88·54

Evaporation.. 0·79 inch.

Rain ....,..... 2·45

By a second gauge. .... 2·48

And at Tottenham....................... .... 2·64

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* * * The mean temperature of the month at Tottenham, with three days' observations, in different parts supplied from the above table, is 33·56°. The thermometer at Tottenham has rather the more open exposure of the two. A fine exhibition of the *Aurora Borealis* was observed there between 11 and 12, p. m. on the 14th of the present month.

Laboratory, Stratford, First Month, 25, 1820.   **L. HOWARD.**

ANNALS

OF

PHILOSOPHY.

_____

MARCH, 1820.

_____

ARTICLE I.

Biographical Account of Stephen Hales, D.D. F.R.S. &c.
Rector of Farringdon, Hampshire, and Minister of Teddington,
Middlesex.

DR. HALES was born in the county of Kent on Sept. 7, 1677.
He was the son of Thomas Hales and Mary Wood. His family
was one of the oldest and most respectable in the whole county,
and his grandfather had been raised to the dignity of a baronet.

He acquired the rudiments of his education in his father's
house. His tutors seem to have confined their instructions to
those branches of knowledge which were considered as connected
with the ecclesiastical profession to which he was destined.
Nothing at this early age announced the great talents which he
had received from nature, unless we consider the assiduity with
which he devoted himself to his studies, and the correctness of
his mode of thinking, as indications of genius. And in fact they
often constitute the dawn of a great man's career, making their
appearance before any other indication of great abilities can be
observed.

He went to Cambridge at the age of 19, and became a pen-
sioner in Bennet College. Here he took his degree, and here
he began to show a fondness for mathematics and physics. He
devoted himself to these pursuits with such ardour that without
any other assistance than his own labour, he made himself suffi-
ciently acquainted with the system of Copernicus to represent it
in a kind of planisphere, in which the planets were placed in
their natural order, and made their revolutions in times propor-
tional to the times of their real revolutions. Such a machine

was at that time but little known, though they have since become very common in England under the name of Orreries ; because the Earl of Orrery employed Mr. Graham to construct one of the first of these machines, which served afterwards as a model for all subsequent ones. A machine of this kind constructed by Mr. Roemer was exhibited to the French Academy in the year 1680.* But Dr. Hales was not aware of any such previous invention when he constructed his own machine.

Dr. Stukely, who afterwards settled in London as a physician, was at that time in Cambridge, and in the same college with Dr. Hales. These two young gentlemen had a similarity of taste which soon rendered them inseparable companions. They traversed the environs of Cambridge in search of plants, with Mr. Ray's Description of the Plants which grow in the neighbourhood of Cambridge, for their guide. This faithful guide often led them into places very little frequented, where they could obtain nothing to satisfy their thirst but sour small beer. This beer Dr. Hales rendered drinkable on the spot by infusing into it a quantity of wormwood or some other bitter plant, to the great astonishment of his hosts. Thus the knowledge cf plants began already to reward him for the trouble which it cost him to acquire it.

To the study of botany was joined that of chemistry ; and our two friends, not satisfied with the ordinary lectures of the professor, repeated in their own apartments various experiments of Boyle, and attended with the greatest assiduity the chemical processes carried on at Trinity College in a laboratory that had belonged to Sir Isaac Newton, and in which the chemical manuscripts of this great man had been burned by a fatal accident. Such was the ardour with which Hales devoted himself to this study, that he seems already to have conceived that he should on a future day have it in his power to repair this great loss to the chemical world.

Anatomy, which is so essential a part of science, was not neglected by our two young gentlemen. Hales made such progress in it that he even contrived a new mode of rendering the vesicles of the lungs visible to the eye. He fixed a gun-barrel to the windpipe still attached to the lungs. This barrel was heated by passing it through a chaffer, and he blew through it, for several hours, into the lungs, a hot dry air, which dried all the membranes and vesicles, keeping them still in a state of distention. He then poured in a quantity of melted tin ; for this metal melts at a temperature so low that when in fusion it does not destroy the texture of animal bodies. When every thing was cold, he destroyed the whole of the lungs by a long maceration. there remained a fine anatomic tree, which not only represented exactly the figure of the interior of the lungs, but enabled him

* See Hist. de l'Acad. tom. i. p. 317.

to form some notion of its total capacity, and of its different cavities. This ingenious idea, which would have done honour to Ruysch or to Winslow, was the fruit of the reflections of a student of anatomy. How great expectations might have been formed of the discoveries of his mature years.

When we consider the astonishing progress which Hales made in every branch of physics, we should be tempted to conclude that he devoted the whole of his time to these pursuits. But this would be a great mistake. His physical studies did not in the least interfere with those which had been the chief motive for his repairing to Cambridge; and he had made such progress in his theological studies that those persons who had the direction of the College being afraid that so promising an associate should escape them, got him made a fellow before the age of 25 years, and though at the time there was no vacant fellowship. He took successively all his degrees, and soon after was named Dean of Ely; so firmly was his ecclesiastical reputation established. It is a difficult task for most persons to acquire a competent knowledge of a single science; but Dr. Hales had abilities sufficient to render himself almost equally well skilled in them all.

As soon as he had taken orders, he was appointed curate of Teddington, in the county of Middlesex; afterwards he got the living of Orlock, in the county of Somerset, which he exchanged for the living of Farringdon, in Hampshire, and in all these situations he performed his clerical duties with the utmost propriety, and to the entire satisfaction of his parishioners. He was elected a fellow of the Royal Society in the year 1718, and the year following he began to read to that learned body some experiments upon the effect of the heat of the sun in elevating the sap in trees. The Society, struck with the importance of his investigations, exhorted him to continue them. He complied with their request; and in the year 1727 published the fruits of his researches under the title of "Statical Essays; containing vegetable Statics; or an Account of some statical Experiments on the Sap in Vegetables, being an Essay towards a natural History of Vegetation; of Use to those who are curious in the Culture and Improvement of Gardening, &c. Also a Specimen of an Attempt to analyze the Air by a great Variety of Chymio-Statical Experiments, which were read at several Meetings before the Royal Society." This work, which immediately raised the author to the very first rank among British philosophers, was dedicated to George II. at that time Prince of Wales.

Few books were ever more favourably received by the public, and few ever deserved a better reception. Most of the experiments were quite new, and the hand of genius was every where conspicuous in them, which is alone capable of opening a new road to great discoveries. He made no attempt to rear any

hypothesis of his own; but stated experiments which completely overturned all the hypotheses previously contrived to account for the motion of the sap in trees. They often led him to surprising conclusions. Could it have been believed, for example, that the force with which a vine branch draws the sap, at the time when the sap flows, is equal to the pressure of a column of water 36 feet in height. Yet Dr. Hales demonstrated this by cementing glass tubes to the extremities of vine branches cut at that season, and observing how high the sap issuing out from the branch rose.

- Similar experiments, but made in a different season of the year, and upon a great number of plants, informed him of the force and the quantity of transpiration of plants, which he contrived to collect and render sensible. The motion of the sap in trees, and even the existence of vessels of communication which allow it to pass laterally from one side to the other, are rendered obvious to the eyes with an inconceivable ingenuity. He estimates the effect of the heat of the sun upon different parts of trees, and that of the temperature of the earth, which he determines to as great a depth as the roots usually reach. He explains the use of the leaves, till that time very little understood, and which, according to him, are the organs by which plants exhale during the day the liquid which they draw from the earth; and which, during the night, on the contrary, draw back again the moisture which they find in the air. This alternate motion in plants is a substitute for the circulation of the blood in animals. It would occupy too much room to specify all the curious experiments related in the first part of this work, and the singular consequences which he deduces from them. But it would be improper to overlook the modest reserve with which he every where restricts himself to a bare statement of facts without hazarding any other conclusions than those which a rigid calculation have converted into certainties. It is unfortunate for the progress of physics that this sage conduct has been too seldom imitated.

The second part of this work is, if possible, still more interesting than the first. It constitutes the real foundation of pneumatic chemistry; for it is doubtful whether Dr. Black, who produced the first great impulse, would have been able to have made out the constitution of limestone had it not been for the previous experiments of Hales. Dr. Black himself informs us that it was the previous experiments of Dr. Hales on limestone, who demonstrated that when this mineral is dissolved in acids, a quantity of air is disengaged from it, that led him first to draw the conclusion, that limestone is a compound of quicklime and air. An examination of the air, which he called *fixed air*, led him to the knowledge of its nature; and the properties of fixed air were afterwards fully investigated by Mr. Cavendish and

Dr. Priestley. This produced the taste for pneumatic chemistry, and of course was the occasion of all the subsequent discoveries in this most fertile region of the science.

Many interesting experiments had been made on air for a whole century before Hales began his career. But hitherto it had been examined only as a heavy, transparent, and elastic body. No one had thought that air was capable of existing in a great variety of substances, deprived of its elastic state, but capable, when favourable circumstances occurred, of assuming all the elasticity of air. Hales demonstrated that almost all vegetable, animal, and mineral substances, contain air, and that the quantity in many cases is astonishingly great. Thus from a piece of oak wood he extracted a quantity of air equivalent to 200 times its bulk, and constituting about a third part of the weight. It is true that Dr. Hales did not suspect that the elastic fluids thus obtained from vegetable, animal, and even mineral bodies, differed entirely in their nature from common air, and consisted of elastic bodies of a very different nature. But it was a great step to call the attention of chemists to the extrication of airs from different bodies. It was natural, after being aware of the fact, that elastic fluids are extricated from a great variety of bodies for chemists to turn their attention to the properties of the elastic fluids themselves. This accordingly was done by Cavendish, Priestley, and Scheele. These bodies were characterized by names according to their characters. The next step was to determine the constituents of these elastic fluids. This was the occupation of Lavoisier and his followers. And of late years, chiefly in consequence of the atomic theory and the theory of volumes, for which we are indebted respectively to Dalton and Gay-Lussac, this important department of chemistry is in a great measure completed. Thus a new and most important chemical rout which was first opened by Dr Hales has only been completely explored in our own day and within these few years.

Hales's experiments on respiration, on the quantity of air deprived of its elasticity by combustion, and by various kinds of vapours, deserve a careful perusal. If he did not see or even suspect the whole truth, he at least greatly facilitated the future investigations of chemists after the composition of atmospherical air, the true nature of combustion, and the important part which oxygen performs in it, were known. This work of Dr. Hales was translated into French by Buffon; and the translation speedily raised the reputation of the author as high on the continent as the original work had already done in Great Britain.

The success of his experiments on the motion of the sap in plants naturally led him to examine the motion of the blood in animals, previously much better known than the former. Accordingly in 1733 he published, by order of the Royal Society, a

collection of his experiments and conclusions under the title of " Hæmastatics, or Statics of the Blood."

He exhibited in fact a measure, and an exact measure, of the force with which the heart forces the blood through the body of the animal. He fixed transparent tubes to various arteries of living animals, and thus observed, by the height to which the blood rose in them, the force with which it was driven by the heart in the different circumstances examined by Dr. Hales. Sometimes the animal was possessed of its full strength; sometimes its strength was diminished by the abstraction of a determinate quantity of blood, or by a variety of other ways which it would be too tedious to describe here. The effect of these changes on the height to which the blood rose in the tube, on the frequency of the pulse, and upon the whole state of the animal, are carefully observed, and from them a vast number of useful and curious consequences are deduced. He shows, for example, that profound inspirations and frequent contractions of the lungs increase the velocity of the blood, and that yawning has the necessary consequence of accelerating the flow of the blood through the lungs. He shows likewise that too great a loss of blood, instead of diminishing, actually increases the rapidity of the flow of blood through the arteries. He extracted the whole blood from animals, and substituted in place of it water heated to the same temperature; but the life of the animal was not supported. Thus he showed that the blood does not act merely as a liquid; but as a substance of a peculiar kind.

His observations on anatomical injections are of great importance, and give quite a new view of that subject. The object of anatomists, in injecting the veins and arteries of animals, is to fill them with a liquid, which afterwards becoming solid, may exhibit these vessels of the same size as they possess in the living animal. But as these vessels are extensible, it is obvious that if the matter of the injection be pushed with greater or less violence than the blood was pushed on in the living animal, the vessel will in consequence be more or less distended than in the living animal, and consequently the capacity of these vessels will no longer be the same as in the living animal. To remedy this inconvenience, Dr. Hales employed, to push on an injection, a column of liquor, which he renders equal in force to that exercised by the heart during the life of the animal. Of the amount of this force he had satisfied himself by his previous experiments. This method of proceeding enabled him to judge with much greater accuracy than former anatomists of the capacity of the different vessels through which the blood flows. He measured the diameters of each, and endeavoured to determine by calculation the velocity with which the blood moves in the different vessels. He determined the facility with which different liquids could pass through the blood vessels, by ascertaining the height

of the column of liquid necessary to drive each on. These experiments led him to observe a very unexpected and surprising phenomenon; namely, that water cannot be made to pass from the arteries into the veins, though blood passes with freedom; and that certain places of these vessels which refuse a passage to water allow beer to pass with the greatest facility. He had measured how much the resistance of the viscera and of the principal vessels of the human body could amount to by determining the height of the column of liquid necessary to rupture them; and he found that this resistance was far superior to the violence to which they had any chance of being exposed. He tried the effects of spirituous liquors, of acids, astringents, emollients, &c. upon the viscera and vessels of animals newly slain. The treatise terminates with a set of experiments on urinary calculi, and on their solvents. These observations and experiments are highly worth the perusal of the physician and chemist. He did not indeed succeed in his attempts to ascertain the nature of these bodies, nor was it possible for him to do so; because destructive distillation, the method which he employed, is not calculated to make us acquainted with the constituents of animal bodies. But his experiments show at least that the previously received opinions upon this subject were erroneous; and had his notions been taken up and followed out, accurate conceptions respecting these bodies would have been much sooner formed than they were. It deserves to be mentioned, to the honour of Hales, that several chemical papers upon the urinary calculi, some published as late as 1789, hardly throw any more light upon the constitution of these bodies than had been already thrown upon the subject by the genius of Dr. Hales.

The Hæmastatics, which constitutes the second volume of the common editions of Hales's Statical Essays, was translated into French by M. de Sauvage, of the Royal Society of Montpellier.

The reputation which Hales had acquired by the publication of these two works successively was so great that the University of Oxford was induced, without any solicitation on his part, to confer on him the title of Doctor in Divinity. This honour is so much the more remarkable because it is not very frequently conferred upon any one who has not been educated at Oxford.

Dr. Hales's experiments having made him acquainted with the effects which spirituous liquids produce upon the blood and the viscera when taken internally, his philanthropy induced him to endeavour to render the knowledge of these effects as extensive as possible. This led him to publish in 1734 a dissertation against the use of spirituous liquids, which he entitled " Friendly Advice to the Drinkers of Brandy." He paints in it the fatal effects of this sort of indulgence in such lively colours as might, one would suppose, be sufficient to deter the boldest drunkard from indulging in his favourite vice. But the unfortu-

nate consequence of this vice is to deprive its victim of the power of reflection. The dissertation of course could not be expected to produce any other effect, except demonstrating the goodness of heart and the patriotism of the author.

Similar motives seem to have led him to examine the nature of sea water, the method of rendering it fresh and drinkable, and of preserving corn, meat, and all sorts of provisions fresh during long sea voyages. These experiments, together with many useful instructions to navigators, constituted a work which he published in 1739, and which he dedicated to the Lords of the Admiralty. For this work the Copleyan metal was voted to him by the council of the Royal Society. The reputation which he had acquired by his work on the urinary calculus, and the utility of that work, induced him in 1740 to examine the nature of Mrs. Stephens's famous cure for the stone, which had just been purchased by Parliament. He endeavours to point out the importance and efficacy of this remedy, which was then at the height of its reputation. But many years have elapsed since that ephemeral reputation has vanished, and now it is admitted by all that no solvent for the stone can be applied without the risk of fully as great an injury to the constitution as that which it proposes to cure.

Three years after, he published a description of the *ventilator* —an instrument by means of which we may at pleasure renew the air in all places where we have occasion to do so. It seems unnecessary to give a description of this simple and ingenious machine, as it is sufficiently known, and as a method of renewing the air in the cabins of ships is very generally used in this country, which may be considered as little else than a simplification of Hales's ventilator. This description of the ventilator was the last separate work published by Dr. Hales. He was then 60 years of age, and did not choose after that period of life to venture upon long publications. But he inserted a great many interesting papers in the Transactions of the Royal Society. Of these, it seems requisite only to notice the most important.

Dr. Berkeley had brought tar water into fashion as a medicine, and its virtues were highly extolled all over Europe. Dr. Hales was too much of a philosopher to be led away by the enthusiastic encomiums passed upon any medicine whatever. He examined tar water, and pointed out the diseases in which it might be used with advantage, or at least innocently, and the cases in which it would be improper to employ it. Tar water has long ago lost its reputation, and is no longer employed in medicine; but Dr. Hales's opinions respecting it ought not to be forgotten, as they show us how far he rose above the prejudices of his age, and the care he took to investigate opinions before he admitted them. In the same year (1745) he pointed out a method of stopping the progress of combustion by covering with a layer of moist earth those buildings through which it was likely the

conflagration would pass. This method was carried to Constantinople, and enabled the Turks to save one of the finest buildings in that Ottoman capital.

Two years before this he had given a method of injecting into the abdomen in the case of paracentesis any injection wished for during the very time that the dropsical liquor is passing off. He had been told that a woman labouring under an ascites had been cured by her surgeon by means of an injection of red wine and alum water. This piece of information led him to think of a method of improving the process. His method was exceedingly simple. Two trocars were introduced into the abdomen instead of one; and while the liquid of dropsy was running out of one canula, the injection was to be introduced through the other. This process has been for many years very much employed in hydrocele, and with the best effects. Its success in ascites is much more problematical.

The phenomena of electricity had attracted the attention of men of science during Dr. Hales's life time; and in the latter period of his existence, they had acquired a very high degree of celebrity, and were become the fashionable study. It was not likely that they could escape the attention of a man possessed of the curiosity and the sagacity which characterized Dr. Hales. Accordingly we find a paper by him on electricity in the Philosophical Transactions. He remarks that the colour of the electric spark is different when drawn from iron, from copper, and from an egg laid upon copper. Hence he infers that the substance from which the electric spark comes furnishes some part of its own substance to the spark, and thereby occasions the peculiarity of colour.

Earthquakes were particularly common and dreadful in their effects during the life time of our philosopher. Hence his attention was naturally called to them, and his ingenuity employed, in attempting to account for their existence. He conceived he had found an explanation in an experiment which had been related in his Statical Essays. He had obtained a quantity of nitrous gas by dissolving iron pyrites in nitric acid, and he found that when this gas was mixed with common air, it became red, and the bulk of the mixture was diminished by a volume nearly equal to the original volume of the nitrous gas. This experiment, which was very imperfectly understood by Dr. Hales, and indeed was not fully explained till many years after his death, he employed to account for the origin of earthquakes. The nitrous gas he considered as an air impregnated with, or consisting of, sulphureous vapours. These vapours in his opinion were continually exhaling from the earth. When they were produced in great quantities, and came to be mixed with pure air, the vacuum formed by the sudden destruction of the elasticity of so great a quantity of air would, in his opinion, be sufficient to occasion earthquakes. Notwithstanding the wildness of this hypothesis,

the paper contains much curious information, and some facts are to be found in it of considerable interest; as, for example, the information that when a cannon is fired in St. James's Park, the glass in the windows of the Treasury becomes charged with electricity.

Lime water had been proposed as a good vehicle for preserving fish, &c. without salting. This method Dr. Hales examined, and found not to answer. He points out its fallacy in a paper printed in the Philosophical Transactions, on the Antisceptic Powers of Lime Water.

The last paper of Dr. Hales which we shall mention contains a description of a method of greatly increasing the rapidity of distillation. The method was to blow up a constant shower of air through the boiling liquid by means of a pair of double bellows. By this method he shows that a very considerable quantity of sea water may be distilled on ship-board, and sweetened in a comparatively short time, and with a small quantity of fuel.

Though the extreme sobriety and regularity of Dr. Hales's mode of living had preserved his health and vigour till a very advanced age, it was not in his power to resist the effects of time. He died accordingly on Jan. 4, 1761, when he had nearly reached his 84th year. He was buried, according to his own request, in his church of Teddington, which he had rebuilt himself a few years before his death. But at the expense of the Princess Dowager of Wales a monument was erected for him in Westminster Abbey.

Dr. Hales had been long known to the Princess of Wales, and had been indeed in some measure in her service. To her consort the Prince of Wales, the father of his late Majesty, he was so well known, and was held in such estimation by him, that this prince was accustomed frequently, alone and unattended, to surprise him in his cabinet, where he was constantly occupied with the most curious and useful researches. He forgot his rank and his grandeur for the pleasure of enjoying his familiar conversation, from which he derived a fund of information which he could not otherwise have acquired. After the death of his Royal Highness, Dr. Hales was appointed Almoner to his consort the Princess of Wales. He had made no application for this place, the whole matter had been transacted without his knowledge, and he could not avoid expressing his astonishment when he heard of his appointment. Modesty indeed was his characteristic virtue. Though known and admired by all the philosophers in Europe, he received the encomiums that were continually lavished upon him, as if he had not deserved them. The same principle rendered him satisfied with his situation, which he considered as fully adequate to his merits. When, through the interest of his friends, the King had appointed him a Canon of Windsor, he employed all his credit with the Princess of Wales to get the appointment cancelled.

He was not more ambitious of literary honours. And in 1753 he was named a foreign associate of the French Academy of Sciences, solely on account of his reputation, without any solicitation whatever on his part. His manners were particularly amiable; and in the cast of his character he is said to have had a very considerable resemblance to the celebrated Boyle. The sole object of all his labours was public utility; and he seems to have had no other ambition than that of discharging his duties to his country and to mankind in general. A single example will be a sufficient specimen of the gentleness and humanity of his disposition. His hæmastatics were not carried nearly so far as his vegetable statics. He assigned the reason of this in a letter which he wrote to M. Duhamel. The tortures, to which he was under the necessity of subjecting the inferior animals, preyed upon his mind so much, that he durst not venture to proceed with his experiments, but found it necessary to drop the subject altogether.

Dr. Hales was married, and always lived with his wife in the most perfect harmony, but it does not appear that he left any children.

ARTICLE II.

Description of a Barometer for measuring Heights.
By Mr. James Allan.

(To Dr. Thomson.)

SIR,   *Erskine, Jan. 26, 1820.*

THE following is a description of a convenient barometer for measuring heights, which I send you for insertion in the *Annals of Philosophy*.

This barometer is of the recurve kind. The turned up part is a piece of wide tube, in which an iron float is to be put to swim on the top of the mercury to prevent its convexity. The proportion of this to the long tube must be accurately ascertained. We shall consider it to be as 12 to 1. A scale of inches, as in other mountain barometers, is to be attached to the top of the tube on the one side, by which to mark the positions of the mercury in the column of suspension. Then from the proportion of the tubes being to one another as 12 to 1, for every 12 parts the mercury falls on this scale, it will rise one part in the turned up tube; so that the column of suspension will be shortened 13 parts. Hence, when the quantity of fall on the scale is correctly found, it is only necessary to add one-twelfth part of itself to it to know how much the column of suspension is shortened.

This, however, may be found more easily by means of a scale,

which we are now going to describe, which may be put in place
of the other. We have supposed the tubes to be to one another
as 12 to 1, and have found that for every 12 parts the mercury
falls on the scale, the column of suspension is shortened 13. If
then an inch be divided into 13 parts, when the mercury in the
long tube falls through 12 of these, the column of suspension
will be shortened a full inch. Hence if a scale be made use of,
whose large divisions are equal to +⅓ths of an inch, instead of
real inches, these nominal inches and their subdivisions will
indicate with accuracy the number of true inches and correspond-
ing parts of inches, by which the column of suspension is
shortened.

In order that altitudes may be found without logarithmic
tables, a scale of the kind we are going to describe, may be
attached to the other side of the barometer. This scale must
be so divided, as to show with accuracy the quantity of fall cor-
responding to certain determinate altitudes. The method of
finding the size of the spaces for the altitudes required is this.
Supposing that it is designed to make the large divisions of the
scale to correspond to 100 fathoms, and that we commence
the calculation from 30 inches of mercury downward, we seek
the logarithm of 30, the decimal part of which is 0·4771, from
this we subtract 100, and there remains 0·4671, which we find
to be the decimal part of the logarithm of 29·32, this we subtract
from 30, and there remains 0·68 of an inch for the size of the
first space. Next, from 0·4671 we subtract 100, and there
remains 0·4571, whose natural number is 28·65, which, when
subtracted from 29·32, leaves 0·67 of an inch for the size of the
second space. In this way are all the divisions and subdivisions
of the scale to be formed.

In finding the spaces above, that I might be able to show the
method more simply, I have made use of logarithms of four
places of decimals only, and the size of the spaces is for that
reason not correct; but, in calculating for the scale, tables
extending to eight or nine places of decimals will be required, as
the utmost accuracy is necessary in its construction. It must
be kept in view that the spaces are not to be taken off a scale of
real inches, but a scale of nominal inches, the same as is second
described.

I come now to show the method of correcting for the tempe-
rature of the mercury, in making use of the above scales. If the
scale of real inches is used, this is done in the common way. If
the scale of nominal inches is used, one-thirteenth of itself is to
be added to the quantity that is to be taken from, or put to, the
length of the column of mercury, because each of the nominal
inches is one-thirteenth of an inch too little. If the scale that
is divided so as to indicate certain determinate altitudes, is used,
a slide which is to extend across both scales is to be screwed
up or down to the place where the mercury should be if the

temperature was proper. This place is easily found on the scale of inches; so that by screwing the slide till the one end come to the proper place on this scale, the other end will of course take the right position on the scale for determinate distances. By adjusting the slide at the foot and top of the mountain, and counting the divisions between its positions, the approximate altitude of the mountain is obtained. As the finding of the true altitude does not depend on the barometer but the approximate only, it is not necessary for me to say any thing about that, as the method is given in every work in which mensuration by the barometer is treated of.

I shall conclude this letter by making a few remarks concerning the most favourable times for measuring with the barometer. Calm weather is indispensably necessary for this operation, because air in motion has not the same perpendicular pressure as when at rest, but its gravity diminishes as its velocity increases. Hence, if a stratum of air of the altitude of any mountain moves with such a velocity as to cause its perpendicular pressure almost to vanish, then the column of mercury, being supported in no sensible degree by that stratum, but by the atmosphere above it, will suffer no apparent change in being carried from the foot to the top of the mountain. Clear unclouded weather is also necessary, for clouds by their shadows and different temperature disturb the regular progression of the atmosphere. Mr. Greatorex, in an account which he gives in the London Philosophical Transactions for the year 1818, of a measurement he made of the height of Skiddaw, ascribes the erroneous results he obtained by the barometer wholly to the effect of clouds.

Beside the above, there is still another circumstance worth attending to, which is the height at which the barometer stands; when it stands high and continues steady, then is the most favourable time for measuring with it. When it is low, it is unfavourable; for the fall may be produced by some approaching wind, giving a buoyancy to the atmosphere before it, or by winds in the higher regions, which circumstances would disturb the geometrical progression of the atmosphere, and hence render a correct measurement impossible.

If all these favourable circumstances which I have mentioned are possessed, and a good barometer used, the results, at the very least, are equally to be depended on as those obtained by trigonometry.   Yours, &c.

                                        JAMES ALLAN.

ARTICLE III.

Results of a Meteorological Register kept at New Malton, in Yorkshire, in the Year 1819. By Mr. James Stockton.

1819. Months.	BAROMETER.						SIX'S THERM.				WEATHER.			WINDS.											RAIN.
	Mean.	Maximum.	Minimum.	Range.	Spaces described in inches, &c.	Number of changes.	Mean.	Maximum.	Minimum.	Range.	Wet.	Snowy.	Hail.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Var.	Brisk.	Boisterous.	Quantity in inches, &c.
Jan.......	29·385	30·31	28·40	1·91	12·62	24	36·855	53	26	27	19	1	—	0	2	2	1	11	7	3	3	2	4	2	3·66
Feb.......	29·322	29·88	28·50	1·38	10·40	19·	37·440	50	25	25	18	3	—	3	0	1	1	4	6	5	5	3	4	4	3·50
March.....	29·672	30·20	28·80	1·40	5·37	15	42·645	55	31	24	15	3	4	5	6	0	0	2	9	6	5	0	3	8	1·40
April.....	29·598	30·18	28·88	1·30	5·74	15	47·000	61	32	29	6			0	8	9	4	5	4	9	1	5	5	1	1·74
May......	29·728	29·97	29·40	0·57	2·90	11	53·550	70	35	35	8			5	5	1	2	2	3	2	2	1	1	0	2·05
June......	29·655	30·05	29·20	0·85	4·02	11	57·433	71	44	27	13			4	1	0	3	6	7	5	1	2	2	3	3·04
July......	29·882	30·12	29·15	0·97	3·64	17	61·774	76	45	31	5			5	8	2	2	5	0	1	7	2	1	0	1·60
Aug.......	29·824	30·20	28·85	1·35	3·33	10	62·420	77	48	29	4			5	12	0	1	2	3	1	0	3	2	1	1·25
Sept......	29·714	30·40	29·00	1·40	4·44	13	54·833	72	36	36			1	7	9	2	0	5	8	4	1	3	6	2	1·54
Oct.......	29·570	30·08	28·95	1·13	5·16	7	48·017	70	30	40	15	1	—	6	4	0	2	5	3	1	6	2	2	6	2·80
Nov.......	29·426	30·02	28·64	1·38	9·44	14	38·560	53	25	28	17	1	1	4	6	0	0	3	4	3	8	2	3	2	2·77
Dec.......	29·420	30·10	28·80	1·30	7·92	15	31·770	54	9	44	8	11	—	3	4	1	1	5	4	4	7	3	7	3	5·50
Annual Means, &c.	29·598	30·40	28·40	2·00	75·55	177	47·773	77	9	68	127	20	6	49	58	18	17	55·58	58	38·44	44	28·44	39	39	30·85

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Sept. 21. Wind, N.	30·400
Lowest ditto, Jan. 25. Wind, S.E. boisterous	28·400
Range of the mercury	2·000
Mean annual barometrical pressure	29·598
Greatest range of the mercury in January.	1·910
Least range of ditto in May.	0·570
Mean annual range of ditto.	1·245
Spaces described by ditto	75·550
Total number of changes in the year	177·000

Six's Thermometer.

Greatest observation, Aug. 14. Wind, W	77·000°
Least ditto, Dec. 31. Wind W.	9·000
Range of the mercury in the thermometer.	68·000
Mean annual temperature.	47·773
Greatest range in December.	46·000
Least ditto in March	24·000
Mean annual range	31·333

Winds.

North and East.	67
North-East and South-East.	75
South and West	93
South-West and North-West	102
Variable.	28

Rain, &c.

	Inches.
Greatest quantity (including snow and hail), in Dec....	5·50
Least ditto, in August	1·25
Total amount for the year	30·85

REMARKS.

The mean and the maximum of the barometrical pressure for the last two years, are nearly alike, and the number of changes in the direction of the column are exactly so; although the spaces described in the former year exceed those of the one just elapsed by $3\frac{1}{2}$ inches.

The mean temperature is about half a degree below that of 1818, and the difference between each month is rather unusual. The first five months and the seventh in 1818 were on an average nearly 4° colder than the corresponding ones in the last year; but the others were proportionably the reverse.

The amount of rain, &c. is two inches less than in 1818.

The principal features of the year are the uniform and long

1

continued heat and dryness of the summer season, and the early commencement of winter, which to the present time has continued with excessive severity.

New Malton, Jan. 4, 1820.   JAMES STOCKTON.

ARTICLE IV.

On the Going of a Clock with a Wooden Pendulum, By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR,   *Bushey Heath, Feb. 7, 1820.*

To determine what reliance could be placed on the going of a clock with a wooden pendulum, I fitted one of my monthly regulators, beating dead seconds, and whose motion continued while winding up, with a straight-grained cylindrical deal rod ; but being dissatisfied with the irregularity of the rate, I was on the point of rejecting this, when, from the circumstance of the clock becoming much out of beat, I concluded the great source of error might probably proceed from the warping of the wood. I, therefore, caused the intermediate portion of the deal, about an inch below that part where the watch spring which suspends the pendulum is fastened to the upper part of the bob, to be reduced from a cylindrical to an eliptical shape, the transverse, or longest diameter being parallel to the back of the clock ; this alteration so much improved the going, that I am induced to trouble you with a table containing the rate for 12 months, and to remark that the rod was perforated in the centre for the crutch to pass through, and a brass eye inserted to prevent the wearing away of the wood. To render the pendulum steady, it was hung independent of the frame that supports the clock ; and the bob, in lieu of being screwed to the rod, was permitted to rest upon a divided nut, turning on a fine screw, attached to the lower extremity of the rod, and which answered the twofold purpose of supporting this weight, and regulating the pendulum. The advantage of permitting the bob to remain unconfined to the rod is, that the expansion of the bob upwards has a tendency to counteract the expansion of that spring by which the pendulum is hung downwards, and of, therefore, preserving the same length. By examining the table, it will be found that the accuracy of the simple pendulum described is little inferior to the compound one, known by the name of the gridiron pendulum ; and when it is also considered that the latter costs 12 guineas, and the rod not as many pence, the variation of the going bears so small a proportion to the difference of price, that it will, generally speaking, be sufficiently accurate for most purposes.   I remain,

My dear Sir, yours, very sincerely,   MARK BEAUFOY,

Table containing the Rate of a Clock with a wooden Pendulum.

Date.	Clock fast or slow.	Differences.	Daily rate.	Date.	Clock fast or slow.	Differences.	Daily rate.
1819.				Aug. 3	−1′ 55·09″	1·29″	−0·43″
Jan. 31	−0′ 10·40″	—	—	7	−1 58·41	3·32	−0·83
Feb. 6	−0 02·07	8·33″	−1·39″	10	−2 01·05	2·64	−0·88
10	−0 03·37	5·44	−1·36	14	−2 03·39	2·34	−0·59
13	−0 07·91	4·54	−1·51	21	−2 07·80	4·41	−0·63
20	−0 18·55	10·64	−1·52	*25	−2 10·88	3·08	−0·77
25	−0 23·47	4·92	−0·98	26	−0 09·75	1·13	+1·43
March 4	−0 30·45	6·98	−0·99	27	−0 09·53	0·22	+0·91
9	−0 38·00	7·55	−1·51	28	−0 09·83	0·30	−0·30
14	−0 46·00	8·00	−1·60	31	−0 07·14	2·69	+0·89
18	−0 50·17	4·17	−1·04	Sept. 2	−0 07·95	0·81	−0·40
22	−0 50 67	0·50	−0·13	6	−0 06·34	1·61	+0·40
24	−0 50·25	0·42	+0·21	9	−0 08·55	2·21	−0·74
26	−0 50·32	0·07	−0·03	13	−0 08·84	0·29	−0·07
29	−0 50·10	0·22	−0·07	17	−0 11·67	2·83	−0·71
April 1	−0 52·02	1·92	−0·64	19	−0 10 33	1·84	+0·67
3	−0 53·86	1·84	−0·92	21	−0 10·75	0·42	−0·21
5	−0 54·20	0·34	−0·17	23	−0 08·55	2·20	+1·10
7	−0 54·34	0·14	−0·07	Oct. 1	−0 20·05	11·50	−1·44
10	−0 55·05	0·71	−0·24	4	−0 18·01	2·04	+0·68
15	−0 57·22	2·17	−0·43	9	−0 18·25	0·24	−0·05
27	−1 08·06	10·84	−0·90	12	−0 21·58	3·13	−1·04
29	−1 08·73	0 67	−0·34	14	−0 23·57	2·19	−1·09
May 1	−1 08·61	0·12	+0·04	16	−0 24·03	0·46	−0·23
4	−1 06·07	2·54	+0·85	18	−0 24·10	0·07	−0·04
7	−1 04·37	1·70	+0·57	22	−0 21·90	2·20	+0·55
9	−1 03·77	0 60	+0·30	27	−0 18 58	3·32	+0·66
12	−1 01·40	2·37	+0·79	Nov. 3	−0 16·60	1·98	+0·28
14	−1 01·70	0·30	−0·15	6	−0 17·22	0·62	−0·21
17	−1 00·15	1·55	+0·52	10	−0 16·50	0·72	+0·18
22	−0 58·70	1·45	+0·29	12	−0 15·66	0·84	+0·42
28	−1 01·64	2·94	−0·49	21	−0 15 56	0·10	+0·01
June 5	−1 00·81	0·83	+0·10	23	−0 13·53	2·03	+1·02
7	−1 02·00	1·19	−0·59	+26	+1 02·58	—	
9	−1 02·85	0 85	−0·42	Dec. 3	+0 59·20	3·38	−0·48
16	−1 04·98	2·13	−0·30	8	+0 57·58	1·62	−0·32
19	−1 07·96	2·98	−0·99	11	+0 57·08	0·50	−0·17
21	−1 10·62	2·66	−1·33	13	+0 57·87	0·79	+0·49
30	−1 19·32	8·70	−0·97	22	+0 47·60	10·27	−1·14
July 3	−1 22·70	3·38	−1·13	24	+0 47·16	0·44	−0·22
7	−1 29·72	7·02	−1 75	27	+0 47·47	0·31	+0·10
17	−1 43·54	13·82	−1·38	30	+0 48·07	0·60	+0·20
19	−1 45·50	1·96	−0·98	31	+0 48·74	0·67	+0·67
22	−1 50·68	5·18	−1·73	1820.			
23	−1 50·91	0·23	−0·23	Jan. 3	+0 49·11	0·37	+0·12
25	−1 53 05	2·14	−1·07	5	+0 48·45	0·66	−0·33
26	−1 54·44	1·39	−1·39	8	+0 49·16	0·71	+0 24
28	−1 54·38	0·06	+0·03	15	+0 48 84	0·32	−0·05
29	−1 54·90	0·52	−0·52	22	+0 45 94	2·90	−0·41
31	−1 56·38	1·48	−0·74	31	+0 37·72	8·22	−0·96

* Aug. 24, clock put forward two minutes.
+ Nov. 25, clock taken to pieces and cleaned.

ARTICLE V.

Translated Extracts from the Persian Work called " The Book of Precious Stones," by Mohammed Ben Manssur. Translated into German by Mr. Joseph Von Hammer.

THERE can remain little doubt but that the knowledge of precious stones first came to us with them from the East; even the names of most of them do not differ from those in the countries where the mines of them are situated; and yet nothing has been made known from these sources except some specimens of the Arabian work of Tëifaschi, which Ravius published in the year 1784, at Utrecht, and some passages in Bochart's Hierozoicon, treating of precious stones. These extracts will, therefore, not be unwelcome, particularly to lovers of mineralogy and precious stones, as they not only contain the original Persian names, but also the very important classification; from which it appears that the fact, that rubies, oriental topazes, and sapphires, belong to one and the same class, namely, that of the Jakut, (which is a modern discovery in Europe,) has long been known to the inhabitants of the East, and that they have been acquainted for centuries with the mode of determining the specific gravity. The author composed his work in the seventh century of the Hegira (in the 13th of the Christian era) for the Emperor Abu Nassr Behadirchan, of the family of Abbas, in two books, the first treating of precious stones, and the second of metals. Considering the ideas that have prevailed in the East for thousands of years, it will not be surprising that among the former the pearl takes the lead.   •

Every chapter regularly consists of four sections, the first of which treats of the external and visible qualities, the second of the mine, the third of the value, and the fourth of the internal mystical qualities. Our extracts are confined to the first two sections of each chapter; as the value set on precious stones in Asia in the 13th century could, at the most, be a useless gratification of the curiosity of amateurs, and an enumeration of their secret, fabulous, and talismanic properties could be of no kind of use to real science.

CHAP. I.

Sect. 1. *Of the Classes of Pearls.*—Pearls are called *merwarid* (hence the Latin *margarita*), or *lulu;* this last name is usually given them when pierced. They are divided into various classes, according to their water and lustre. 1. *Schahwar*, i. e. Royal pearls, the brightest and purest. 2. *Dürr*, the common pearls, likewise called *choschab, nedschmi,* and *ojun.* 3. *Schekeri*, i. e. sugar pearls, are of a red and yellowish colour. 4. *Benini*, yellow-white. 5. *Serdi*, the yellow-red. 6. *Kebudi*, the blue-

white. 7. *Rossassi*, those clouded with a kind of lead colour. 8. *Surchab*, those watered with red. 9. *Siahab*, those watered with black. 10. *Schemii*, the wax-coloured green and yellow, and not transparent. 11. *Rochami*, the marbled, dark, not transparent, and without lustre. 12. *Chusckkab*, those of dull water, in contradistinction to those called *choschab* (mentioned above), i. e. of pure transparent water. With respect to their form they are divided, 1. Into the *mudahredsch*, those quite round. 2. *Ghabni*, those of the egg shaped. 3. *Aakid*, half flat and half round. 4. *Scheldschami*, turnip-shaped. 5. *Adsi*, lenticular. 6. *Seituni*, in the form of an olive. 7. *Schairi*, shaped like a barley corn. 8. *Seili*, formed like a tail or train. 9. *Schemi*, in the form of a taper. 10. *Fokai*, in the form of a Can. 11. *Nimrui*, hemispherical. 12. *Mussarres*. With respect to their size, they are divided into 15 classes, according to the number of the sieves through which they are passed, and of which one has always larger holes than another. The pearls of the first sieve, which has the smallest holes, are called : 1. The twelve hundred ; because 1200 of them weigh a *miskal*. 2. Those of the second sieve, the five hundred. 3. The four hundred. 4. The three hundred and fifties. 5. The three hundreds. 6. The hundred and eighties. 7. The hundred and seventies. 8. The hundred and sixties. 9. The hundred and fifties. 10. The hundred and twenties. 11. The hundreds. 12. The eighties. 13. The seventies. 14. Fifties. 15. The forties, 40 of which weigh a *miskal*.

Sect. 2. *Of the Pearl Fisheries.*—The best are at *Serendib* (Ceylon), and in the gulph of Persia at *Bahrëin, Kisch,* and *Scharek,* but the Arabian are less valued than the Indian ; their colour and quality depend on the bottom of the sea where they are produced ; they become dark in a black mud, and yellow in a shallow sea. The pearl oysters drawn out of the sea sometimes move very quickly, and sometimes not at all.

Chap. II.

Sect. 1. *Of the Properties of the Jakut.**—It is of six different kinds : 1. The red. 2. The yellow. 3. The black. 4. The white. 5. The green, or peacock colour. 6. The blue, or smoke-coloured. The first, namely, the *red*, is again subdivided into six kinds : 1. *Wirdi*, the rose-coloured. 2. *Erghiwani*, the purple-coloured. 3. *Behremani*,† the yellow-red. 4. *Lahmi*, the flesh-coloured. 5. *Sumaki*, the porphyry-coloured. 6. *Remmani*, the pomegranate-coloured. The second kind, the yellow,

* It cannot be doubted that the *jakut* is our sapphire (télésie), and it is astonishing that the orientalists had already, at that time, a proper idea of this stone, for which we are indebted to the latest researches, which particularly coincides with the division into four classes: the *red* (rubis d'orient), *yellow* (topaze d'orient), *blue* and *white*.

† Behreman is an Indian flower, and, as some will have it, the blossoms of the Carthamus.

has three divisions: 1. *Mischmischi*, the apricot-coloured.
2. *Narendschi*, the orange-coloured. 3. *Kahi*, the straw-coloured.
The third and fifth kinds (the *black* and *green*), and the second
and fourth kinds (the *yellow* and *white*), are one and the same.
The sixth class (the *blue*) consists of four kinds : 1. *Asrak*,
the light-blue. 2. *Ladschiwerdi*, the azure colour. 3. *Nili*,
the indigo-coloured, each of which has several subdivisions.
Some divide the jakut into four classes : into the red, yellow,
dark, and white, as they count the peacock-coloured, and the
blue among the dark. The jakut cuts all stones, except cor-
nelians and diamonds,* and can only be cut by the diamond. -

Of other precious stones, only the *Laal*, of Bedachschan, has
the lustre of the jakut ; it is harder than all other stones, and
cool in the mouth ; the red jakut appears white in the fire, and
again attains its former colour when taken out of it. When it is
cut, it is called *memsûh*, and in its original state *adschemi*.
There are six kinds of precious stones similar to the red jakut.
1. The *Laal*. 2. The *Bidschade*. 3. The *Benefsch*. 4. The
Kerkend. 5. The *Kerkin*. 6. The *Kuser*. The *kerkend* is of a
dark-red colour, and the *kerkin* reddish-black, and transparent
in the sun. The *kuser* has all the colours of the various kinds
of the *jakut*. The difference between the *jakut* and the stones
that resemble it is, that it scratches them, is heavier, and bears
the fire. Thus the white *Jakut* weighs more than the crystal,
which it often resembles.

Sect. 2. *Of the Mines of the Jakut*.—On the island of Saha-
ran, which is 62 farsanges in diameter, and lies about 40
farsanges behind the island of Ceylon, is a high mountain called
Sahun, in which jakuts of all colours are found. In the year of
the Hegira, 669 (A. D. 1270), a mine of *Jakut* was discovered to
the east of the village of *Tara*, in the third climate, and in the
same latitude as the Canary Islands, and half a day's journey
from Cairo, though some people assert that there is no *jakut*
mine except the mountain of *Sahun*.

CHAP. III.—*Of the Emerald.* (*Semerrüd*.)

Sect. 1. *Of the Properties of the Emerald.*—It is divided
according to its colour. 1. Into the *subabi*, grass-green.
2. *Rihani*, basilisk-green. 3. *Suluki*, leaf-green. 4. *Sindschari*,
dirty-green. 5. *Kerassi*, euphorbia-green. 6. *Assi*, myrtle-
green. 7. *Sabuni*, soap-green. The grass-green is of a beautiful
light colour, like the green worms which are often seen in the
grass ; it is the lightest, as the soap-green is the darkest. The
emerald, according to the degrees of its purity, is also divided
into the bright polished (*saikali*) and the dark (*sulmani*). The
first reflects every thing that is held before it like polished steel,

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* This statement of its hardness and weight characterizes it with the most preci-
sion. Though the oriental carnelian is uncommonly hard, and difficult to polish,
this far too high estimate of its hardness is a singular but pretty general error.

while the latter does not bear the fire so well. The difference between the emerald and stones resembling it, as the jasper, the green *laal* and *mina* (green glass), consists in the polish. The oblong emerald is called *kasaba* (staff), and several pieces of emerald joined together by *mina* (green enamel), are called *astar*.

 Sect. 2. *Of the Mines of the Emerald.** —On the borders of Negroland is a pit of emeralds which still belongs to Egypt, where they are dug first out of talc, and then out of a red earth. The soap-green emerald is also found in *Hedschas*, and it is on that account called the *Arabian*.

<center>CHAP. IV.—<i>Of the Chrysolite.</i> (<i>Seberdsched.</i>)†</center>

 Sect. 1. *Of the Properties of the Chrysolite.*—*Abunassr Farabi*, and many other learned philosophers, do not consider it to be of any particular species, but a kind of emerald: it is more beautiful and clear, and is divided into three classes; namely, 1. The dark-green. 2. The middle-green. 3. The pale-green.

 Sect. 2. *Of the Mines of the Chrysolite.*—It is dug out of the same mines as the emerald, and seems to be composed of the same materials, but less finished.‡ *Tëifaschi* says, that in his time no chrysolite was dug; the rings which are seen of them come from Mauritania, and tradition considers them as fragments of the treasures of Alexander, who sought in the deserts of Africa for the fountain of life. After he had penetrated with his army into the land of darkness, in which flows the green fountain of life, it is said that the gravel under their feet (green, with the reflection of the fountain of life) was called the pebbles of repentance (*hassbaen-nedamet*). When they returned to the light, this saying was confirmed; for both those who had gathered none of the pebbles, and likewise those who had gathered some, repented, the first, because they had nothing, the second, because they had only chrysolite, and which was on that account called *the pebbles of repentance*.

<center>CHAP. V.—<i>Of the Diamond.</i></center>

 Sect. 1.—There are seven kinds of it. 1. The white-transparent. 2. The pharaonic. 3. The olive-coloured, the white of which inclines to yellowish. 4. The red. 5. The green.

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 * It is very interesting to learn, with some degree of precision, the oriental mines of the emerald, to be able to explain where the Greeks and Romans, of whom we have indubitable works in emerald, procured this stone, as they could not be acquainted with the only place where they are now found, the valley of Peru. From the latest accounts of the Frenchman, M. Caliot, who had been sent by the Pascha of Egypt to look for the ancient emerald mines, he has been so fortunate as to discover them in the neighbourhood of the Red Sea, which pretty nearly coincides with these accounts.

 † Ravius merely translates the *Seberdsched* as Smaragdum minoris valoris in his Latin treatise, because Tëifaschi, as appears from the text, merely considers it as a kind of emerald.

 ‡ The difference between the emerald and the chrysolite, both in their external as well as chemical characters, is now sufficiently known, and also that, according to modern travellers, the chrysolite is found in Syria.

6. The black. 7. The fire-coloured. The first two kinds are the most common, the others more rare, and that which is quite polished, the most seldom found. It does not break on the anvil under the hammer, but rather penetrates the anvil. In order to break it, it is laid between lead, which is struck with the hammer, and then it breaks. Others enclose it in resin, or wax, instead of lead. The diamond has an affinity with gold, small particles of which are attracted by it; it is also much sought for by the ants, and covered with them, as if they would devour it. In India, where it is very highly esteemed, the exportation of it was formerly prohibited.

Sect. 2. *Of the Diamond Mines.*—In the eastern part of India is a deep ravine inhabited by serpents,* where diamonds are produced. Some people suppose that it is found in the *jakut* mines.

CHAP. VI.—*Of the Cat's Eye.* (*Ainol-hurr.*)

Sect. 1. *Of the Properties of the Cat's Eye.*—It is a brilliant transparent stone, which appears to the spectator like the eye of a cat seen in a light place. If you turn the stone, this bright focus also turns; and if light falls on it, it plays in waves, which move the more, the stronger the light is which falls on it; if you break a cat's eye into pieces, you find the same focus in every one of them.

Sect. 2. *Of the Mines of the Cat's Eye.*—It is affirmed that the cat's eye is found in the jakut mines, and formed of the same matter.

CHAP. VII.—*Of the Spinell.* (*Laal.*)†

Sect. 1. *Of the Properties of the Spinellus.*—It is of four different kinds: 1. Red. 2. Yellow. 3. Violet. 4. Green, like the emerald. Sometimes the same stone is half green and half red. The red is of eight kinds: 1. *Geschdimegi.* 2. *Piasegi.* 3. *Temeri*, the date-like. 4. *Lahmi*, the fleshy. 5. *Anabi*, the dove-like. 6. *Bakami*, having the colour of Brazil wood.‡ 7. *Edrisi*, the stone enoch. 8. *Ekheb*, the dark. The *geschdimegi* is remarkable for its pleasing colour and lustre. The *piasegi* has derived its name from the village of *Piaseg*. The flesh-like is dark-red. The gradations of the spinell are various, and jewellers know very well that there is sometimes no difference in the colour between the. spinellus, the garnet, and the coloured crystal. The difference consists in the superior hard-

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* Here the well-known fable is mentioned, out of the thousand and one nights, of the birds which fetched up pieces of meat to which the diamonds stuck.

† It is not to be doubted that *laal* is our spinellus, which is found in all shades of red, and several of violet and bronze, as also green, like *pierre de Mahomet* : as yellow, or under a denomination of red, the author, perhaps, took the.hyacinth, which has much resemblance, both in its brilliancy and the manner of treating it for the purpose of polishing.

‡ M. Von Hammer has here the word *Pernambukartige*, which it seems difficult to translate otherwise; yet Brasil was not known to the Persian author.

ness of the spinellus, which is not broken on the anvil, while the coloured crystal, when held to the sun, appears white. The *laal* had its name from Bedachschan, not so much because it is found there, as because it is sold in that province.

Sect. 2. *Of the Mines of the Spinell.*—At the time of the Caliphate of the Abbassides, a mountain at *Chatlan* was rent open by an earthquake, where there was found the laal of Bedachschan bedded in a white stone. It is very hard to polish, and it was a long time before it could be smoothed,* till it was at length accomplished by means of the *gold marcasite* called *ebrendsche.* Smaller stones are found in the bed round a large one, like the seeds of a pomegranate. The miners call this bed of the spinell *maal.* There were found in the mines first red, then yellow laal, and it belongs to the kinds of the *iakut.*

Chap. VIII.—*Of the Turquoise.*

Sect. 1. *Of the Properties of the Turquoise* (Firuse).—It comes 1. From *Nischabur.* 2. From *Ghasna.* 3. From *Irak.* 4. *Kerman.* 5. From *Chowaresm.* The first is the most valued, on account of its hardness, purity, and durable colour. This has seven kinds: 1. *Abu Ishaki.* 2. *Esheri.* 3. *Suleïmani,* a milky and sweet stone. 4. *Sermuni,* with golden spots. 5. *Chaki,* sky-blue. 6. *Abdol-medschidi,* beautifully coloured, but soft. 7. *Andelibi,* a little milky. The turquoise is bright or dull, according to the weather; † and is larger in rainy days than in fair. One kind of it becomes of a more beautiful colour in oil, ‡ but then loses it again. Jewellers call it *mescha;* that of two colours is called *ebresch.* The turquoise is also similar to a kind of green and blue enamel. According to the time in which it was dug up, it is divided into the old and new mines, of which the new change the colour.§

Sect. 2. *Of the Mines of the Turquoise.*—It is found in those places after which it is called; the most beautiful and richest mines are at *Nischabur,* where that called after *Abu Ishak* is the most beautiful, and the *andelibi* the faintest.

Chap. IX.—*Of the Bezoar (Pasehir) and other Animal Stones.*

Sect. 1. *Of the Properties of the Bezoar.*—It is of two kinds: 1. The animal. 2. That found in mines. The latter is divided

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* The spinell is extremely difficult to polish, which can only be effected by oil of vitriol on a copper-plate. And it is very remarkable that the author mentions, instead of the oil of vitriol used by our lapidaries, the *marcasite* (iron pyrites), from which the oil of vitriol may be produced.

† These are probably limestone coloured by vitriol of copper.

‡ The blue of the turquoise is in reality of so delicate a colour that the influence of the light on a bright or gloomy day seems to cause a striking change in it.

§ Some turquoises frequently change the shade of their colour, which probably is caused by their inferior hardness and porosity, and from the effects of acids, or imbibing of oily particles: thus in a ring with four blue turquoises one turned green after an illness of its wearer.

into: 1. The yellow. 2. The green. 3. The dust-coloured. 4. That spotted like a lizzard. 5. The whitish, spotted with gold spots. They make of it chessmen, draughtsmen, handles for knives, and the like. If you throw the green bezoar into the fire, it turns black without being burned; the inhabitants of Kerman call it *muchati schëitan*. It is the contrary with the animal bezoar; it is likewise sometimes green, sometimes yellow, sometimes of a dust colour, may be easily powdered, and assumes a white colour when it is powdered on the stone. It is divided into the cow bezoar (*bakari*), and into the sheep bezoar (*schati*). The former is a soft yellow stone; the latter, green and soft. It is very often counterfeited; the real may be distinguished from the false, as the former will not take a mark of fire, as its colour does not fall into a blueish, as it has no dots, and, when rubbed, gives off a white colour.

Sect. 2. *Of the Mines of the Bezoar.*—It is found on the borders of India and China, as also between *Mossul* and *Dschesirei Ben Omer*. It is said that the animal bezoar is produced in China in the eyes of the stags, in which the exhalations of serpents, which they have devoured, precipitated by the water, are said to be condensed into bezoar. The sheep bezoar is said to be produced in the stomachs of some sheep on the frontiers of Persia.

C.IAP. X.—*Of the Cornelian (Akik)*.

Sect. 1. *Of the Properties of the Cornelian.*—It has seven kinds: 1. The liver-red. 2. The rose-red. 3. The yellow.* 4. The white. 5. The black. 6. The blueish. 7. That of two colours. Though a hard stone, it is much used for engraved seals.

Sect. 2. *Of the Mines of the Cornelian.*—It is found in *Sanaa* and *Aden*, in *Yemen*, on the frontiers of India and *Rum*, and in the neighbourhood of *Bassra*.

CHAP. XI.—*Of precious Stones resembling the Jakut, viz.* 1. *The Benefsch†* (Violet). 2. *Bidschade* (Garnet). 3. *Badendsch*.

Sect. 1. *Of the Properties of the Benefsch.*—It is of four different kinds: 1. *Madeni*, of a pure bright transparent red colour, quite similar to the red jakut; so that if it is strung with the jakut upon the same thread, the best judges can scarcely distinguish them. 2. *Ruthi*, garlic. 3. *Benefschschi*, blackish-red. 4. *Istasescht*, of a light-yellow colour. All kinds of the benefsch‡ have an affinity with the laal, but the benefsch inclines more to blue than the laal.

* Yellow is called *serd* in Persian; and here, and not in the city of Sardes, we are to look for the origin of the name of the sardonyx.

† Benefsch, Bidschade, and Madendsch, are certainly only different shades of the garnet, and may probably be the violet (almandin), the dark-red, and the yellowish-red oriental garnet; namely, that of Ceylon and Syria. That their specific gravities are very different is well known.

‡ Ruvius translates Benefsch by amethyst, as falsely as he does jakut by hyacinth.

Secondly, *Bidschade*, the garnet, is a red stone, of pure water, which often loses its lustre when worn in the dress,* and which is distinguished from the jakut not only by its inferior weight, but also by a greater degree of warmth, the jakut, when taken into the mouth being cold, and making it moister, while the contrary takes place with the garnet. Thirdly, the *madendsch*, or *madebendsch*, is a very red stone ; it is quite similar to the garnet, but its red inclines more to black, and it is lighter in weight. It has no lustre till it is cut deep from below.†

Sect. 2. *Of the Mines of these Stones.*—The benefsch is found in the mines of the spinell ; the garnets and madendsch (madebendsch, or madenidsch) are found on the frontiers of *Bedach-schan*, and brought to *Cachemire*, about ten days' journey off, which has given rise to the erroneous supposition that there were mines of them at Cachemire. The garnet has a division like the laal, and is found in the mountain of Sahun, where there are also jakut mines. When they come from the mines, they are dark, and without water, and are not bright and transparent till they are cut.

Chap. XII.—*Of the Onyx (Dschesi).*

Sect. 1. *Of the Properties of the Onyx.*—There are several kinds, as : 1. *Bakrawi*. 2. *Habeschi*. 3. *Anebi*. But they are classed according to their colour : 1. Into the white. 2. Into the black. 3. Into the red. 4. Into the particoloured. The *bakrawi* has three layers ; the first, red, and not transparent; the second, white and transparent; the third, transparent, like crystal. The *habeschi* has likewise three layers, two dark, and a white one in the middle. The onyx is the hardest stone after the diamond or jakut, and is about the same weight as a cornelian. Some onyxes are striped, others not; in others, the stripes are interrupted ; so that they form singular figures.

Sect. 2. *Of the Mines of the Onyx.*—Though the onyx is found in several places, the most esteemed are those found on the frontiers of China and Arabia.

Chap. XIII. *Of the Magnet.*

Sect. 1. *Of the Properties of the Magnet.*—There are four kinds of the magnet : 1. The *iron* magnet, commonly called the *iron robber, ahenruba*. 2. The *gold* magnet. 3. The *silver* magnet. 4. The *tin* magnet, which attracts gold, silver, and tin. The magnet loses its power in oily substances, but increases it when put into blood, gold, or vinegar. The silver magnet is a white light stone, which swims on water, attracts silver, and is com-

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* As they have little hardness, they soon become dull.
† It is also usual among us to cut garnets of a dark colour hollow, or to lay foil under them.

monly called *hadschrol-bokur,* i. e. cow's-stone. The gold magnet is a pale yellow stone, which attracts gold, and the tin magnet is a heavy sinking stone, which attracts tin.

Sect. 2. *Of their Mines.*—They are found in Arabia, India, and other places.

CHAP. XIV.—*Of the Senbade.** (Quæry Spar?) (German Spath?)

Sect. 1. *Of the Properties of the Spar.*—It is a hard stone, which polishes iron and steel. It is distinguished from stones which resemble it by its hardness, which is next to that of the diamond, which alone scratches it. It is either reddish or blueish.

Sect. 2. *Of the Mines of the Spar.*—It is found in many places, as in India, Zanguebar, Siwas, Kerman, Nubia, and Ethiopia. The best comes from Nubia and Siwas.

CHAP. XV.—*Of the Malachite (Dehne).*

Sect. 1. *Of the Properties of the Malachite.*—The malachite is a green stone, which has the colour of verdigrease, with red and black spots. Some persons affirm that in *Turkistan* a red malachite, of the colour of the red jakut, is produced. The *dehne* is of five kinds: 1. The leek-green. 2. Basilisk-green. 3. The black-green. 4. The white-green. 5. The emerald-green. The pure malachite is called the sweet (*schirin*), and the dull the bitter (*telch.*) This is only valued very much in Syria and Europe; when it is smeared with oil, it receives additional lustre; when it is old and much worn it loses its beauty, and the white of its spots turns yellow. It appears like the turquoise bright in serene weather, and in cloudy, dull. If you rub it with natron and oil, you obtain the purest copper.

Sect. 2. *Of the Mines of the Malachite.*—It is found in five places: in the mountains of Mauritania, in Kerman, in Haske-rek, near a city which was built by Efrassiab, in Turkistan, and in Arabia, in the cavern of the Beni Salem.

CHAP. XVI.—*Of the Lapis Lazuli (Ladschiwerd).*

Sect. 1. *Of the Properties of the Lapis Lazuli.*—Its four kinds are; namely, 1. *Bedachschi.* 2. *Gurdschi.* 3. *Dermari.* 4. *Ker-mani.* The first, i. e. that from *Bedachschan,* is divided into that with gold spots, and into that without. Powdered lapis lazuli thrown into the fire produces a many-coloured smoke.

Sect. 2. *Of the Mines of the Lapis Lazuli.*—The most remark-able of them is the lapis lazuli mountain in *Chatlan,* near *Bedachschan,* but it is also found in Georgia, in Kerman, and in other places.

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* Senbade is most probably the diamond spar, or corundum; and the word spar (spath) is more likely derived from senbad than from spähen, from which Adelung derives it.

Chap. XVII.—Of the Coral (Bessed and Merdschan.)*

Sect 1. *Of the Properties of the Coral.*—There are four kinds of corals : 1. The red. 2. The white. 3. The black. 4. The dark-coloured. They are soft and white as long as they are in the water, only become hard when out of the water, and assume different colours : the genuine can be distinguished from the counterfeit by the smell of the sea weed ; in oil, they become beautiful and shining, but in vinegar soft and white. They are very much valued in China and India, because they are used for adorning the idols. *Tëifaschi* relates that he had seen a smelling bottle made of a coral, a span and a half long, and three fingers broad.

Sect. 2. *Of the Places where they are found.*—They are generally fished up in the Mediterranean Sea. The best are the reddest, and the largest of a straight stem. They are polished with spar, and bored through with steel of Damascus.

Chap. XVIII.—Of the Jasper (Jascheb, or Nassb).

Sect. 1. *Of the Properties of the Jasper.*—It has five kinds : 1. The white and light. 2. The whitish-yellow. 3. The black-green. 4. The transparent black.† 5. The dust colour. In China, they make a false jasper, which is distinguished from the genuine by its smoky smell. If a vessel of genuine jasper breaks, it is repaired with artificial pieces, which are scarcely to be distinguished from the natural.

Sect. 2. *Of the Mines of the Jasper.*—In China there are two mines of it, of which the one called *Ak Kasch* produces light jasper, and the other called *Kut Kasch*, dark. The large pieces belong to the Emperor, the smaller to the workmen. Jasper is also found on the frontiers of *Kaschgar*, in *Kerman*, and *Arabia*.

Chap. XIX.—Of the Crystal (Bellor).

Sect. 1. *Of the Properties of the Crystal.*—It is more pleasing, pure and clear than other precious stones, and is of two kinds : 1. The clear and pure. 2. The dark-yellowish. It can be melted like glass, and then coloured in imitation of the jakut, laal, or emerald. *Tëifaschi* relates, that in his time a merchant of Mauritania was in possession of a bath made of two pieces of crystal, which was so large that four persons could sit in it. In the Treasury of *Gasna* there were four crystal vessels, each of which contained two skins (*borachio*) of water. *Abu Rihan* mentions the assertion of the lapidaries that there was often found in crystal wood, and the like, and that he himself had seen two crystals, in one of which was enclosed a green twig, and in the other a hyacinth.

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* Some say that *bessed* is the Persian, and *merdschan* the Arabian word for corals; others, that the former signifies the stem, and the latter the branches.

† The transparent black jasper may, perhaps, be the obsidian, if it is known to oriental naturalists.

Sect. 2. *Of the Mines of the Crystal.*—The crystal is found in seven places: In India, Turkistan, Europe, Arabia, China, Armenia, and the remotest frontiers of *Moghrib* (Mauritania). Some prefer the Arabian to the Indian, but the least valued is the Armenian, which is called *rim-bellor.*

CHAP. XX.—*Of the Amethyst (Dschemest).*

Sect. 1. *Of the Properties of the Amethyst.*—The amethyst has several colours, like the rainbow, and four kinds: 1. Deep rose-coloured and sky-blue. 2. Pale rose-coloured and deep azure. 3. Pale rose-coloured and sky-blue. 4. Deep rose-coloured and pale sky-blue. The Arabians set an extraordinary value upon the amethyst, and adorn their arms ·with it.

Sect. 2. *Of the Mines of the Amethyst.*—It is found in the environs of the village of *Safwa*, about three days' journey from *Medina.* Wine drunk out of a goblet of amethyst does not intoxicate.*

CONCLUSION.

Of various other Stones.

Sect. 1. *Of the Weschich, or Schebak (probably Jet).*—It is a black stone, easily broken, which reflects objects. It is of two kinds: the Indian and the Persian; the former is better than the latter.

Sect. 2. *Of the Chamahen.*—It is called the ass's stone; it is very hard, and can only be bored by the diamond; when broke it divides into branches; and when rubbed on a hard stone, colours it red: the most beautiful is the blackish-red; it is found in the district of *Karak.*

Sect. 3. *Of Talc (Talk).*—It is of two kinds: that produced in the open air, and that found in mines. It is called *sitarei semin,* i. e. star of the earth, on account of its clearness and lustre. Artificial pearls are made of it, which are scarcely distinguishable from the natural. They may be known from each other by this, that the artificial swim on the water, but the genuine sink. The talc does not burn nor calcine in the fire. If you dissolve it and rub the limbs with it, it makes them fire proof.† It is found in many places; the best in Cyprus. The talc can neither be pounded in mortars, nor broken to pieces with iron hammers. The way to dissolve it is to boil it with beans, to wrap it then in

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* To this opinion of the amethyst, which is current also in Europe (by which it has gained the honour of being used as a test or touch stone) it seems to owe its Persian name, in which we find the name of *Dschem* or *Dschemschid,* whose goblet is said to have consisted of a single amethyst. The Greek name Αμεθυςτος is also unintoxicated, but it ·is originally to be derived from *Dschemest,* as the jasper from *Jascheb,* the hyacinth from *Jacut,* the emerald from *Semerrud,* pearls (Margaritæ) from *Merwarid,* the turquoise from *Firuse,* the lapis lazuli from *Ladschiwerd,* the sardonyx from *Sard,* talc from *Talk,* chalk from *Kals,* &c. &c.

† If this is confirmed, it is probably the secret of the incombustibility of the Dervise Rufaji, who performed all kinds of tricks with a red-hot iron.

a piece of linen, and to beat it till it is dissolved, and oozes like milk through the linen. If dissolved talc is mixed with a little resin and saffron, and used as ink, it makes a gold ink, and without saffron, silver ink.

Sect. 4. *Of the Rainstone.*—A soft stone, of about the size of a large bird's egg, which is much celebrated among the Turks. It is of three kinds : 1. The dust coloured with red and white spots. 2. The dark-red. 3. The various coloured. Some persons consider it as a production of a mine ; some as an animal stone, which is said to be found in the stomachs of swine, or in the nests of some large bird. The Turkomans affirm that they can produce rain and snow with this stone.

Sect. 5. *Of the Eagle Stone.*—If you shake it you hear it rattle as if there were something in it ; and on breaking it, you find nothing in it.

Sect. 6. *The Jarakan* (*the Jaundice Stone*).—It is a stone with red and yellow spots, which, when it is rubbed, leaves a red mark. It is so hard that it can only be bored through with the diamond ; a little black stone which the swallows carry into their nest to cure their young of the jaundice.

Sect. 7. *The Vinegar Stone.*—It attracts vinegar, but cannot remain in it, as it always flies out when thrown into it.

Sect. 8. *The Oil Stone* is set in flames when water is poured over it, but it is extinguished with oil.

Sect. 9. *The Jew's Stone.*—A shining stone, which is produced in the sea, and has three kinds : 1. The round. 2. That in the shape of a nut. 3. The oval ; is often marked with black stripes, is hurtful to the stomach, but very useful to the bladder.

Sect. 10. *The Milk Stone,* which, when rubbed, leaves a white mark ; it is ash-coloured, and has a sweet taste.

Sect. 11. *The Mouse Stone,* which has the smell of mice.

Sect. 12. *The Blood Stone, also Schadendsch, i.e. Lentil Stone.* —This last kind is used to polish the surface of the eye (den spiegel des auges damit zu glätten).

Sect. 13. *The Moon Stone,* a stone with spots, which become larger and smaller with the increase and wane of the moon.

Sect. 14. *The Colour Stone,* which always reflects different colours.

Sect. 15. *The Sleep Stone,* which produces sleep when hung over the bed.

Sect. 16. *The Stone Miskal,* which is said to be thrown up from the Mauritanian Sea.

Sect. 17. *The Marcasite, likewise called the Stone of Brightness,* is divided into several kinds : the gold marcasite is dug up near Ispahan, and is called Ebrendsche ; it is used to polish the spinellus : the silver marcasite comes from the frontiers of Bedachschan ; the copper and iron marcasite is similar to copper and iron.

Sect. 18. *The Magnina* (*Manganese?*) which is used by the glass manufacturer. It is divided into that with little and into

that with large shining spots; but according to the colour into
the blackish, yellowish, and reddish.

Sect. 19. *Of the Sürme and Tutia (query Antimony ?).*—It is a
bright, heavy, transparent, black stone, which is divided accord-
ing to the country where the mines are situated, into those of
Ispahan, Herat, Sabulistan, Georgia, and Kerman. The first is
the best, the last the worst; if powdered Sürme is applied to the
eyes, it increases their polish. The tutia (the genuine eye-paint),
is divided into those of Kerman, Kand, India; the last is pure
and white like salt; that of Kerman yellowish. It is made by
laying the natural tutia stone upon coals, and catching the
vapour in an alembick upon nails. The lightest tutia, and the
best for the eyes, is that which forms on the points of the nails,
the second sort on the middle, and the coarsest sort on the
heads of the nails. The Indian is produced on the shore of the
sea, and is much used in alchemy.

Sect. 20. *Of the Proportions of some precious Stones to others.*
—*Abu Rihan* is said to have found by experiment that a *miskal*
of blue jakut is equal in size to five dank * and three *tissu* of
red jakut, or to five dank and two and a half *tissu* of laal; and that
four dank minus a *tissu* of coral are equal in size to four dank minus
two *tissu* of onyx and crystal. The mode of discovering the size and
weight is the following: a vessel is filled with water, and the
stones thrown singly into the water; the quantity of water which
is expelled from the vessel by means of each stone is equal to
the room it occupies. God knows best.

ARTICLE VI.

*Description of an Apparatus for the Analysis of organized
Substances.* By W. Prout, M.D. F.R.S. &c.

THERE is nothing new in the principle upon which the analy-
tical process is conducted by the following apparatus. The sub-
stance to be analyzed is introduced into a glass tube, G (Pl. CII),
(about ¼th or ⅟₆th of an inch in diameter, and 10 inches long)
mixed with the requisite quantity of the black oxide of copper,
precisely in the same manner and with the same object explained
by me in a former paper on this subject.† The tube above-men-
tioned is inserted firmly into a piece of cork at its upper and open
end in such a manner that about half an inch of it may project
beyond the larger end of the cork. The cork is then placed in
the conical hole in the piece of brass, C (fig. 2), fixed in the

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* According to Meninski, a *dank* is equal in Egypt to three carats; according to
Cassira, two in Spain. It is the fourth part of a drachm, but according to Fer-
heng the sixth. The *tissu*, according to Ferheng, weighs sometimes two, sometimes
four barley corns; and the *miskal* is one drachm and a half.

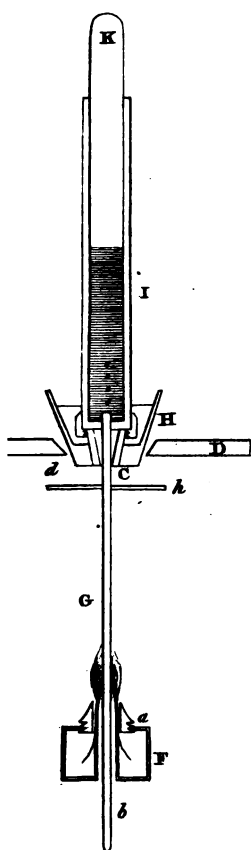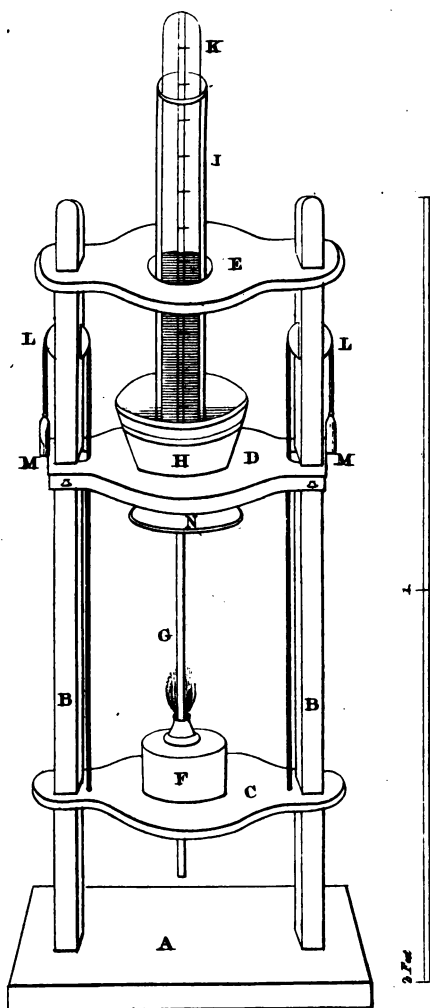† *See Med. Chirurg. Trans.* vol. viii. p. 526.
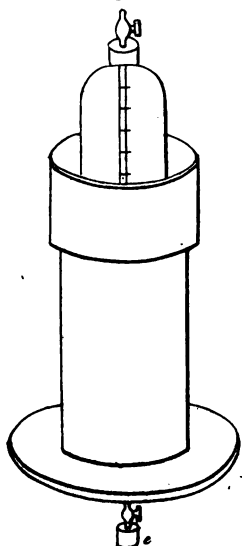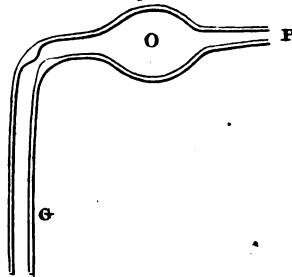
Fig 2   Fig 1

Fig 3

Fig 4

bottom of the wooden dish, H, and the dish is placed upon the support, D, which has a hole in its centre adapted for receiving it. F, is a spirit lamp with a circular wick, like the common Argand lamp. This lamp stands upon a support, C (fig. 1), capable of being moved up and down by means of the counterpoize weights, M M, attached to the lines passing over the pulleys, L L, in a manner easily understood by a bare inspection of the figure. The tube, G, passes through the centre of the lamp, and thus is enveloped equally on all sides by the flame. The gaseous products are collected in the graduated tube, K, which had been previously filled with mercury, and inverted in the wooden dish above described likewise partly filled with mercury. The external glass tube, I, is furnished with a brass screw cap by which it can be attached at pleasure to the piece of brass fixed in the bottom of the wooden dish, in the manner shown in fig. 2. The use of this tube is to afford an easy means of equalizing the height of the mercury on the inside and outside of the tube, K, and thus to supersede the necessity of calculation. N is a circular tin plate, with a hole in the centre, of such a size as to admit the tube, G. This plate is suspended by wires from the support, D ; and its use is to prevent the action of the flame of the lamp upon the wooden dish, H, and its contents. Fig. 3 is a small mercurial gasometer, which may be used instead of the dish and graduated tube when it is desired to take the specific gravity of the gaseous products. The tube, G, in this case is fixed into the brass cap, *e*, by means of a cork. When the above apparatus is employed, the lamp, F, is raised to the upper part of the tube, G (two or three inches of which at this part is filled with pure oxide of copper only), and there permitted to stay till the tube becomes red-hot. When this is the case, it is depressed a little, and another portion of the tube similarly heated, and so on, till the whole of the tube has been heated in succession, when the operation is completed. The gaseous products are then analyzed in the usual manner, if the substance submitted to the operation has contained azote ; if not, the whole of the gas (except a very minute quantity) will be carbonic acid.

The proportion of hydrogen in a substance may be ascertained in several different ways by means of this apparatus. A mode I have commonly practised is (after filling it as usual) to exactly counterpoise the tube, G, in a delicate balance, and when the process is finished to see how much it has lost in weight by the operation. The quantity of gases produced by the same substance being previously known, the quantity of hydrogen may be thus readily estimated. Another mode is to actually collect the water formed, and to weigh it. This may be effected by having the tube in the shape represented in fig. 4. At the end of the operation, most of the water will be found collected in the part O, and to ensure the collection of the rest, another tube filled with dry muriate of lime may be attached to the end, P. Another

mode is that recommended by Mr. Porrett. The quantity of gases being ascertained as usual, the oxide of copper employed in the experiment is to be put into sulphuric acid. The portion of the oxide which has been reduced is thus obtained in a metallic state, and consequently the quantity of oxygen which has been expended may be thus ascertained. Of these methods, the first appears the most simple and least liable to error.

The above apparatus is susceptible of far greater precision, and is much less troublesome to use than any that has hitherto been recommended for the analysis of organized substances. There may be cases in which its use is inapplicable, but these I am persuaded are few.*

In conclusion I may observe that I have for several years been engaged in the analysis of organized products, and have at length extended my researches to almost every distinct and well defined substance. The results, when compared with one another, are most interesting, and seem to throw no small light not only on the nature of chemical compounds in general, but upon many important points connected with animal and vegetable physiology and pathology.

ARTICLE VII.

ANALYSES OF BOOKS.

Memoirs of the Literary and Philosophical Society of Manchester,
Second Series. Vol. III. 1819.

(*Concluded from* p. 141.)

VI. *On the refractive Powers of Muriatic Acid and Water.*
By Mr. Henry Creighton.—The author, during a course of experiments on the application of different fluids to the formation of compound lenses, with a view to correct aberration, was led from Dr. Blair's observations to notice particularly the effects of muriatic acid. He was surprised to find that when such a compound lens was used, the focal distances were proportional to the specific gravity of the muriatic acid employed. The following table exhibits the focal distances with acid of different strengths. The first column exhibits the specific gravity of the acid employed, the second column the focal distances, and the third column the specific gravity calculated from the supposition that it is proportional to the focal distance. The focal distances of the two double convex lenses were about 24 and 27 inches respectively; when placed together in the frame about 13;

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* The annexed sketch represents the apparatus just as it was originally constructed. Perhaps it might be improved by having, instead of the dish, H, an oblong vessel furnished at one end with a deep well for equalizing the height of the mercury on both sides of the graduated tube, K, in which case the external tube, I, as well as the gasometer, fig. 3, would be unnecessary.

when the space between them was filled with water, nearly 24; and when muriatic acid of the specific gravity 1·177 was introduced, the focal distance was 28 inches.

Liquids.	Sp. gr. of ditto by experiment.	Focal distances.	Sp gr. deduced from focal distances.
		Inches.	
Water.	1·000	23·75	1·000
Ditto with a little acid.	1·055	25·00	1·053
Ditto with more acid.	1·087	25·70	1·088
Ditto, ditto, ditto.	1·121	26·60	1·121
Ditto, ditto, ditto.	1·146	27·00	1·138
Muriatic acid.	1·177	28·00	1·180

The lenses in these experiments were made of crown glass. The experiments were repeated with lenses of flint glass and crown glass; but the results were the same. On trying the same experiments with nitric and sulphuric acid, he found that the specific gravities increased at a much greater rate than the focal distance. From this remarkable property of muriatic acid, the author suggests the use of such a compound lense to determine the specific gravity of the acid.

VII. *An Essay on the Origin of Alphabetical Characters.* By the Rev. William Turner, Jun. A.M.—It has been pretty generally maintained by literary men that the first alphabet was made known to mankind by Divine revelation. Dr. Hartley was of opinion that it was revealed from Mount Sinai when the ten commandments were written by the finger of God on two tables of stone. The same notion was supported by the celebrated Gilbert Wakefield in an ingenious paper on this subject inserted in the second volume of the Memoirs of the Manchester Society. Mr. Turner's object in the present essay is to combat this opinion, and to show that letters, like many other discoveries, not inferior in difficulty and importance, may have been the fruit of human sagacity properly directed. The essay is divided into two parts: in the first, he endeavours to answer the arguments advanced by Mr. Wakefield in support of the Divine origin of letters; in the second, he gives his ideas of the way in which the discovery may have been made.

Mr. Wakefield's first argument is, that the invention of letters differs in one remarkable particular from all others; namely, that the first effort brought it to perfection. This assertion Mr. Turner is disposed to deny. We have no evidence that it was perfected at once. The first rude attempts would be forgotten in consequence of the more perfect ones that followed them. Even the Hebrew alphabet seems at first to have been very rude. If the Phœnicians borrowed their written language from the

Hebrew, the Hebrew alphabet must have consisted at first of only 16 letters.

Mr. Wakefield's second argument is, that if alphabetical writing was the result of human ingenuity, we might reasonably have expected to hear of the invention having been made in more places than one ; but this is not the case. All the alphabets at present in existence may be traced either by external or internal evidence to the same source. This Mr. Turner admits. All the European alphabets (the Russian excepted, who got it immediately from the Greek) may be traced to the Roman. The Roman was derived from the Grecian, and the Grecian from the Phœnician. The Coptic, Ethiopic, and Arabic alphabets are referable to the same quarter. But that this fact, though curious and remarkable, furnishes proof of the Divine origin of letters Mr. Turner denies ; because the same thing may be said of several other arts which yet have never been alledged to be of Divine origin. For example, the nine digits. All Europe derived them from Spain. The Spaniards got them from the Moors, the Moors from the Arabians, and the Arabians from the East Indies — a region in which many of the arts and sciences flourished in a very remote period of antiquity.

Mr. Wakefield's third argument is, the uniform failure of all those nations who have continued for a great length of time unconnected with the rest of the world in their attempts to devise any contrivance similar to the alphabetical characters, or at all comparable to them in simplicity and convenience; though they have made considerable proficiency in various other arts and sciences. But Mr. Turner replies that the failure of the Chinese in inventing an alphabet is no more surprising than that such acute people and such mathematicians as the Greeks should have failed in contriving numeral characters comparable to the Arabic in simplicity and utility.

Mr. Wakefield's fourth argument is, that the transition from hieroglyphics to letters, which has been commonly supposed, is by no means an easy or obvious thing. This Mr. Turner admits, but denies that it is any argument in favour of the Divine origin of letters. Though they were not derived from hieroglyphics, they may have been invented in another way.

In the subsequent part of this paper, the author states his objections to Mr. Wakefield's opinion, and endeavours to give an idea how the discovery of letters might have been made.

His objections are: 1. The want of necessity for such a supposition. 2. The total want of historical information on the subject, which could hardly have been the case had letters been derived from the immediate revelation of God. Dr. Hartley's notion that they were first communicated by God in the ten commandments cannot be true, because writing is spoken of before the delivery of these commandments (Exodus, xvii. 14),

and because the ten commandments do not contain all the letters in the Hebrew alphabet.

Our author supposes that when men set about devising methods of committing language to writing, two modes would suggest themselves : 1. To suggest to the eye by visible signs the ideas expressed by spoken language. This would introduce hieroglyphics, from which the characters of the Chinese would be naturally derived. 2. To devise a system of visible signs corresponding to the words used in spoken language. These marks would be gradually systematized; similar sounds would be denoted by similar marks. When these marks multiplied, the idea of ascertaining the number of sounds in the language, and applying a mark for each, would naturally suggest itself, and the transition from this to letters was comparatively easy.

VIII. *Observations on the Rise and Progress of the Cotton. Trade in Great Britain, particularly in Lancashire and the adjoining Counties.* By John Kennedy, Esq.—This interesting paper contains no information about the introduction of the cotton trade into Lancashire. We know from acts of parliament that it existed there during the reign of Queen Anne, though probably to a very limited extent. From a note added to the paper, we learn that Mr. John Wyatt, of Birmingham, invented a spinning machine in the year 1733, in a small building near Sutton Coldfield. He afterwards joined with Lewis Paul; but the project turned out unfortunately. Paul contrived to get a patent taken out in his own name in 1738 for some additional apparatus. In 1741 a mill turned by two asses walking round an axis was erected in Birmingham, and 10 girls were employed in attending the work. But this establishment was unsuccessful, and the machinery was sold in 1743. A work on a larger scale on a stream of water was established at Northampton under the direction of Mr. Yeoman, but at the expense of Mr. Cave. It contained 250 spindles, and employed 50 pair of hands. But this new establishment was also unsuccessful ; and as no model of Mr. Wyatt's machine remains, its nature and principles are at present unknown.

The operation of weaving still remains nearly the same as it did a century ago, with the exception of the fly shuttle, which was invented in 1750 by Mr. John Kay, of Bury. At that time the cotton was carded and spun in the weaver's family, and the manufactory was carried on to an extent sufficient to supply a limited home consumption. The occasional fluctuations in the demand from bad seasons, scarcity of food, &c. led the manufacturer to endeavour to find a market for his goods in other countries. This gave rise to the foreign trade with all its advantages and disadvantages. The demand increased beyond the ability of the manufacturer to supply. This occasioned an incessant demand for new hands, and led to the contrivance of every expedient to make the work done by their labourers as

great as possible. A division of the labour was first thought of
Instead of carding, and spinning, and weaving, in the same
house, one family's sole employment became carding, another's
spinning, and that of a third weaving. The attention of each
being thus occupied by fewer objects, he was able to perform a
greater quantity of work than before. Gradual improvements in
the mode of carding and spinning occasionally suggested them-
selves till at last they arrived at a machine, which, though rude
and ill constructed, enabled them to produce more in their
respective families. By degrees, the cottages became filled with
their little improvements till they were in some measure forced
out of their dwellings by the multiplication of their implements.

Thus carding and spinning became two distinct professions.
First, a boy or a girl was enabled by means of two pair of stock-
cards to do more work than before. A cylinder revolving on its
axis was next introduced, and carding was performed by holding
four or five pairs of stock-cards against it. This was the rude
beginning of the carding machine, and in this state it existed
about 60 years ago. This, about 10 years after, was followed by
another machine, called the spinning Jenny, invented in 1767
by Mr. Hargreaves, of Blackburn, by means of which a young
person could work 10 or 20 spindles at once.

This machine was at first turned by the hand; but horses
were soon substituted for human labourers; and when the size
of the machines came to be such as to render the application of
horses expensive and troublesome, falls of water were sought out
and resorted to.

It was at this time that the admirable inventions of Mr. Ark-
wright were introduced into the cotton trade. The comforts and
independence of the workmen had gradually improved, and with
them the nature of their mechanical inventions. They were
enabled to employ smiths, carpenters, and millwrights to realize
their ideas, and by their superior skill they were enabled to make
their machinery much better suited to the objects intended than
their own original and imperfect fabrics. The inventions of
Arkwright were of a higher kind; and to realize them, the
assistance of a still higher class of mechanics, watch and clock-
makers, whitesmiths, and mathematical instrument makers, were
called in and employed. Mr. Arkwright's first mill was built at
Cromford in 1771, and for a period of 10 or 15 years after, all
the principal works were erected on the falls of considerable
rivers, no other power but water having been then found to be
practically useful. It was not till about the year 1790 that Mr.
Watt's steam-engine began to be understood, and introduced in
the neighbourhood of Manchester. In consequence of the
introduction of this admirable machine, water-falls became of
less value, and instead of carrying the people to the power, it
was found preferable to place the power among the people
wherever it was most wanted. This led those who were inter-

ested in the trade to make many and great improvements in their machines and apparatus for bleaching, dyeing, and printing, as well as for spinning. Had it not been for this new accession of power and scientific mechanism, the cotton trade would have been stunted in its growth, and, compared with its present state, must have become an object only of minor importance in a national point of view. The effects of this engine have been nearly the same in the iron, woollen, and flax trades.

In the year 1780 a new and valuable machine appeared, called at that time the *hall-in-the-wood machine*, from the name of the place where the inventor, Mr. Samuel Crompton, lived, near Bolton, in Lancashire. It is now called the *mule*, from its uniting the principles of Mr. Hargreave's jenny, and Mr. Arkwright's water frame. This machine, by producing at a small expense, much finer and softer yarn than any that had been seen before, gave birth to a new and most extensive trade. Before the year 1790 the mules were turned by hand, and were confined chiefly to the garrets of cottages. About that time Mr. Kelley, of Lanark, first turned them by machinery. The application of the steam-engine to this purpose produced another great change in this branch of the trade. The mules were removed from cottages to factories, were constructed more substantially and upon better principles, and produced yarn of a more uniform quality and at less expense.

In 1797 a new machine for cleaning cotton was constructed by Mr. Snodgrass, and first used at Johnston, near Paisley, by Messrs. Houston and Co. This was called a scutching or blowing machine. It was first brought to a state of perfection by Mr. Dunlop, of Glasgow. It was not introduced into the neighbourhood of Manchester till 1808. It is now generally employed, and is said to have been greatly improved by Mr. Arkwright and Messrs. Strutts.

What are called *power looms* were first constructed by Dr. Cartwright, at Doncaster, in 1774. But though they made good cloth, in consequence of the great loss of time in dressing the warp, they possessed no important advantage over common looms. In 1803 Mr. Thomas Johnson, of Bradbury, Cheshire, invented a beautiful and excellent machine for warping and dressing warps preparatory to weaving, by which the operation is performed much better and cheaper than it can possibly be done by hand. This is a great advantage to the power loom, and in consequence some large manufactories of the kind have been established first in Scotland, and afterwards in England. But as one person cannot attend upon more than two power looms, it is still a doubtful question whether this saving of labour counterbalances the expense of power and machinery, and the disadvantage of being obliged to keep an establishment of power looms constantly at work ; while in the common way the looms might be stopped or turned to a different kind of weaving,

if the demand for the particular kind of goods they were weaving should change or fall off.

Such is a sketch of the historical facts contained in this interesting paper. I regret that I cannot touch upon the observations which it contains respecting the alterations in the morals, the habits, and the feelings of the common workmen produced by, or accompanying these, improvements. They deserve the particular attention of statesmen and political economists. Unless means can be fallen upon to prevent that degeneracy which has hitherto been the lot of all the manufacturing population in every country, it is obvious that all manufacturing countries must sooner or later work their own destruction. The following is the quantity of raw cotton consumed or converted into yarn in Great Britain and Ireland during 1817:

Total imported	110,000,000 lbs.
Loss in spinning ($1\frac{1}{4}$ oz. per lb.)	10,312,500 lbs.
Yarn produced	99,687,500 lbs.
Number of hanks (at 40 per lb.)	3,987,500,000
Number of spindles employed (each producing two hanks per day, and 300 working days in the year)	6,645,833
Number of persons employed (supposing each to produce 120 hanks per day)	110,763
Number of horses power employed (supposing $4\frac{1}{4}$ oz. coal to produce one hank of the 40 and 180 lbs. coal per day = one horse power)	20,768

IX. *Memoirs on a new System of Cog and Toothed Wheels.* By Mr. James White, Engineer.—Mathematicians have demonstrated that the form of the teeth of wheels, in order to move equably and without friction, should be regulated by the curve called the epicycloid. But this holds only when the pins of the pinions which they put in motion are indefinitely small, which never can hold in practice. This circumstance has prevented workmen from attempting to make the figure of the teeth of their wheels correspond with the theoretical figure. The object of the present paper is to make known a method of cutting the teeth of wheels so as that they shall give smooth and equable motion, and as little friction as possible. For this method, a patent was taken out some years ago by the author of the paper.

X. *On the Flexibility of all Mineral Substances, and the Cause of Creeps and Seats in old Coal Mines.* By Mr. John B. Longmire.—The author has observed a very great flexibility in sandstone and slate clay in excavated coal mines. He has likewise been certain varieties of limestone and clay-slate bend considerably when in the act of being separated into small parts by wedges and other tools. From these facts, he infers that all

kinds of mineral matter, however hard and brittle they appear to be in hand specimens, will bend less or more when formed into large flat pieces. He explains what are called *creeps* in old coal mines. This name is given to the slow subsidence of roofs of old coal mines. When the roof sinks rapidly, the process is called a *set* or *seat* by the colliers.

XI. *Account of the Black Lead Mine in Borrowdale, Cumberland.* By Mr. Jonathan Otley.—This mine, according to Mr. Otley, lies in a greywacke mountain near the head of the valley of Borrowdale, which faces the south-east. The black lead does not constitute a vein, but occurs in *bellies.* Veins of iron ore traverse the mountain, exhibiting traces of black lead, and it is where these veins cross each other that the bellies usually occur.

When the mine was discovered is not known; but from a grant made in the beginning of the 17th century, it appears to have been discovered before that time The manor of Borrowdale is said to have belonged to the Abbey of Furness, and having, at the dissolution of that monastery, in the reign of Henry VIII. fallen to the crown, it was by James I. granted to William Whitmore and Jonas Verdon, including among other things, *the Wadholes and Wad, commonly called Black Cawke, of the yearly Rent or Value of Fifteen Shillings and Fourpence.* The said William Whitmore and Jonas Verdon, by a deed bearing date Nov. 28, 1614, sold unto Sir Wilfred Lawson of Isel, and several inhabitants of Borrowdale, all the said manor of Borrowdale, with the appurtenances of what nature or kind soever; " except the wadholes and wad, commonly called black cawke, within the commons of Seatollor, or elsewhere, within the commons and wastes of the said manor." In consequence of this reservation, the black lead mine is held distinct from other royalties of the manor; one half thereof belonging to Henry Banks, Esq. M.P. and the other half subdivided into several shares, one of which belongs to Sir Joseph Banks.

The mine used formerly to be wrought at irregular intervals, and when the market was supplied, the working was stopped till a fresh demand called for a new supply of black lead. But of late years the demand has increased so much that it has been necessary to work the mine without intermission. In the year 1798 an adit was begun on the side of the hill, which at the length of 220 yards communicated with the bottom of the old workings. Through this level the water passes off, and the produce is brought out to be dressed: and on its mouth a house is built where the overseer dwells, and the workmen are undressed and examined as they pass to and from work. From a belly opened in 1803, 500 casks of black lead of the best quality were procured, weighing about one hundred weight and a quarter each; besides a greater quantity of an inferior sort. Since that time, two of these bellies have been met with, which have produced about 100 casks each. The whole is sent to London, where it

is exposed to sale in the Company's warehouse on the first Monday of every month.

The great consumption is in the manufactory of black lead pencils. Many of these are made at Keswick; but the makers are obliged to bring all the black lead from London. Great improvements have been introduced into the manufactory of these pencils. A method of hardening the black lead has been discovered. This, according to Mr. Otley, consists in reducing the black lead into slices about $\frac{1}{45}$th of an inch in thickness, and then keeping it for some time in melted sulphur.

XII. *Account of a White Solar Rainbow.* By the Rev. R. Smethurst.—This rainbow was observed on Nov. 28, 1816, about two, p. m. There was a considerable fog on the surface of the earth, which was clearing away in the higher regions of the atmosphere. There were no visible drops of rain. The sun was visible through the fog; but its rays were not sufficiently powerful to occasion shadows of objects. The whole rainbow was very well defined, except a small portion at the centre. The distance seemed 100 yards, and the span of the arch about 120 yards. Its breadth was about twice that of an ordinary rainbow; its colour grey; near the ground the colour was brighter than towards the centre. In each leg, about an equal distance from each edge, was a streak of white, reaching apparently to the height of 16 or 18 yards, of peculiar brightness.

XIII. *Remarks (chiefly agricultural) made during a short Excursion in Westmoreland and Cumberland, in August,* 1815. By John Moore, Jun. Esq.—This is a very amusing and interesting account of the objects which struck the attention of the author during an excursion to the lakes. It will repay the perusal of the agricultural reader. But from the nature of the details, they are scarcely susceptible of abridgment. From Bolton to Blackburn the soil seems to be poor, and the farming bad. The author thinks that too much of the soil has been ploughed up, and informs us that it is difficult to recover the grass after it has been once destroyed. He considers the planting of whole potatoes a bad practice; and thinks that in 30 years the growth of the oak overtakes that of the larch. Hence, in his opinion, oak is a more profitable tree for planting than larch. He was surprised to observe promising crops of oats and barley growing upon Latrig, at a height, according to Mr. Otley, of 1100 feet above the level of the sea; but he doubts how far the spirited experiment will remunerate the farmer. He recommends bringing the seed corn rather from a colder than a warmer climate; because it ripens much sooner. The wheat straw near Lancaster was of the same beautiful colour as on the chalk lands of Bedfordshire. But it does not appear that this straw is put to the same use as at Dunstable, where it is not unusual for a crop of wheat straw to sell for more than the grain.

XIV. *A Tribute to the Memory of the late President of the*

Literary and Philosophical Society of Manchester.   By William Henry, M.D. F.R.S. &c.—This paper has been already published in the *Annals of Philosophy*, vol. xiv. p. 161.

XV. *An Essay on the Signs of Ideas, or the Means of conveying to others a Knowledge of our Ideas.*   By Edward Carbutt, M.D. Physician to the Manchester Infirmary, &c. &c.—By *idea* the author of this paper means the remembrance of a sensation. He is of opinion that the sensations of the eye and ear may be remembered, but is doubtful whether this be the case with those of the nose, the mouth, or the feeling. A remembered sensation or idea is always fainter than the sensation itself, except in cases of insanity.

Words are usually considered as the signs of ideas. But our author is of opinion that we very seldom succeed in conveying our own ideas into the minds of our hearers or readers. It is impossible for us to have a name proper for each individual idea or set of ideas. On this account, names have been generalized; and when the appellations *tree, horse, man*, had been given to one individual of each of these kinds, they naturally enough came to signify any individual of that kind. Hence the original of general terms, which, by a strange perversion, have been stated to stand for general ideas; whereas a general idea cannot possibly have any existence; all ideas are and must be particular.

The author conceives that there are four sets of words which in no wise represent ideas.

1. Words which, from the nature of things, are altogether devoid of archetypes. Thus the mathematical terms *point* and *line*, our author thinks cannot possibly convey an idea to the mind. This mode of speaking depends upon our author's definition of *idea*, which restricts its meaning more than is usual in the English language. A *point*, in the mathematical sense of the word, is the place where one line terminates and another begins. Of this I have an accurate notion. But whether it be the remembrance of a sensation will admit of some dispute. The same remark applies to the mathematical word *line*, which means the place where one surface terminates and another begins.

2. Words standing not for ideas; but either for simple and original sensations as are never *ideally* renewed, or for sensations of emotion, which are never called up, except by the original cause, and cannot, therefore, in any case be called ideas. When we reason concerning such sensations, we merely employ the words, which are the names of the original sensations; and from these words the author thinks that we can reason just as correctly as from real ideas. I must acknowledge that I do not see the full force of this distinction. It appears to me to be rather fanciful than real. Any person who has been accustomed to smell a *rose* will immediately recognise the same smell when

a bottle of otto of roses is held to his nose. How could this be the case if he did not remember the smell of a rose ; that is, if he had not an idea of the smell of a rose, according to our author's definition of idea. In the same way we can remember tastes and feelings, and even particular kinds of painful or pleasant feelings, and recognise them again whenever they occur. This shows clearly that our author's distinction is fanciful.

3. Words standing for causes whose effects alone we witness, and thence judge the existence of the cause, without being able to form any conception of it. The terms *heat* and *cold*, when used for material agents, our author considers as of this kind. I do not perceive the accuracy of this opinion any more than of the preceding. I have just as good a notion of *heat* as a *cause* as I have of any other cause whatever. With respect to the Supreme Being, it is true that we can form no precise notion of his shape or size ; but we can form pretty exact ones of his power, his wisdom, his goodness, &c.

4. Words which at present have no ideas attached to them, although the contrary might have been the case when they were originally brought into use. In consequence of the existence of such words, our author thinks that a man cannot understand English without the knowledge of all the languages from which it is derived. Now I must acknowledge that (laying unmeaning oaths aside, which are now banished polite conversation) I do not believe that any such words occur in the English language. Our author gives *blasphemy*, as an example, and *heresy* as another. Now the meaning of the word blasphemy in the New Testament is an attempt to lower the character of God, knowing the allegation to be false. Now this is the meaning which it bears also in the English language. *Heresy* now signifies religious opinions contrary to those established by law. It is likewise the term which a man (supposing no religion established by law) would apply to every set of religious doctrines contrary to his own. In this way might every English word be defined, and a mere Englishman, if he were to make himself acquainted with these definitions, would understand his own language better than the most learned etymologist in the universe. Would an Englishman understand the meaning of the word *physician* any better by being told that it was derived from a Greek word which signifies a *naturalist*?

XVI. *Account of some remarkable Facts observed in the Deoxidation of Metals, particularly Silver and Copper.* By Samuel Lucas, Esq.—According to Mr. Lucas, when silver or copper is kept melted in contact with the air, the metal absorbs a portion of oxygen, which it retains while in fusion ; but on the metal becoming solid, the gas is extricated with considerable rapidity, and occasions an explosion if the extrication takes place by pouring the melted metal into water. The gas extricated from silver in this way was examined by Mr. Dalton, and found to

contain 87 per cent. of oxygen gas. The impurity was probably owing to an accidental mixture of atmospheric air.   Though it is difficult at present to give any explanation of this singular phenomenon, is it not possible that it may be somehow connected with the extrication of oxygen gas when silver is placed in contact with deutoxide of hydrogen ?

When the silver or copper is covered with charcoal, and then thrown in fusion into water, no gas is extricated from it.

XVII. *Observations on the Callous Tumour.* By Mr. Kinder Wood.—This is a name given to a tumour which surrounds the fractured part of a bone, when it is uniting together, and which continues till the adhesion is complete, or till a perfectly bony cement has perfectly united the two fractured portions of bone. Mr. Wood's observations were deduced from fractures made on the limbs of young rabbits. These were allowed to remain without any attempt to replace the bones, and the progress was ascertained by dissection at various distances of time after the fracture.

The fracture occasions a loss of vital energy, and about 12 hours elapse before any visible attempt at repairing the injury commences.   The ends of the fractured bones are at first rough, which occasions them to injure the muscles in their neighbourhood.   The first change that takes place is the effusion of a quantity of coagulable lymph from the internal membrane of the bone.   By this, the fractured faces are covered, and made beautifully smooth.   This lymph adheres firmly to the internal periosteum, and obviously proceeds from it.

About 36 hours after the fracture most of the extravasated blood is absorbed. The external membrane of the bone becomes thickened and vascular, a quantity of coagulable lymph exudes from this membrane and from the muscles, and surrounds and envelopes the ends of the fractured bone. It is this exudation which constitutes the callous tumour. It becomes gradually firmer and firmer, the external periosteum of the bone below it is destroyed and removed, and a new periosteum formed which encloses the callous tumour, and fixes it firmly to the bone. The earthy salts are slowly deposited in this tumour, and in proportion as the bone acquires strength, the tumour is diminished ; but a long time elapses before it disappears.

When the external periosteum of the bone is destroyed, no callous tumour is formed, the ends of the bone lose their vitality, and must be removed before the fracture can be healed. When the internal periosteum is injured or destroyed while the outer remains, the callous is formed as usual ; but the bones do not unite, because their asperities are not removed by the usual deposition of coagulable lymph. The consequence is, that the callous gradually assumes the form of a capsular ligament, the inner periosteum gives out a glairy fluid resembling synovia, and an artificial joint is formed.

XVIII. *On the Possibility of reconciling the scriptural and profane Accounts of the Assyrian Monarchy.* By the Rev. John Kenrick, A.M.—From the Old Testament, it appears that the monarchies which existed in that part of Asia, which was afterwards the seat of the Assyrian monarchy, were originally very small; for a king of Shinar, confederated with a king of Elam and two others, came and attacked some petty sovereigns of Palestine; and Abraham, arming his own household, was able to defeat the invaders. David had a war with the King of Tobah, which was at so short a distance from Nineveh, that had that city been the seat of a great monarchy, Tobah could not possibly have been independent, nor could David have attacked it without coming into hostilities with the sovereign of Nineveh. The first mention of conquering monarchs at Nineveh in the Jewish annals is in the reign of Pul, 771 years before the Christian era, by whom and his successors, Syria and Palestine were invaded, and the two branches of the Jewish people reduced to dependence and captivity (2 Kings, xv. 19). Such is the antiquity and uninterrupted series of the Jewish annals, and such the position of the Jewish nation relatively to a power on the banks of the Tigris, extending its dominion westward, that we may safely say that had any such power existed previously to the eighth century before Christ, it must have come into collision with the Jewish nation, and that collision would have been recorded in Jewish history.

From Herodotus (whose Assyrian history is lost) we learn little more than that the Assyrians had been masters of Upper Asia (Asia beyond the Halys) 520 years, when the Medes shook off the yoke; which (reckoning backwards from 710 years before Christ) brings us to 1230 years before Christ. But this account, though improbable, is not inconsistent with the scriptural history; since the Assyrians might be masters of Upper Asia without holding the sea coast.

It is from Diodorus Siculus that the usual account of the early Assyrian monarchs is obtained. Ctesias is his authority. He was a Greek, and posterior to Herodotus, and even by many of the ancients his history is looked upon as fabulous. According to him, Ninus, a prince of boundless ambition, engaged the Arabians under his standard, subdued Babylonia, Armenia, and Media, and proceeded thence to remoter countries of Asia, which he reduced, with the exception of Bactria and India. Thence, turning his arms towards the west, he overran all the countries from the Euphrates to the Mediterranean, and from the banks of the Tanais to the cataracts of the Nile. After these exploits, he builds Nineveh, which he names from himself. He then returned to his former unsuccessful attempt on Bactria, and deprived Menon, the chief officer of his army, of his wife, Semiramis, making her his own queen. Semiramis succeeded Ninus. She founded Babylon, and spent the rest of

her days in warlike expeditions and public works. After an unsuccessful expedition into India, she died miraculously, leaving the kingdom to her son Ninyas, who began a course of effeminate sloth and luxury, which his successors followed for 30 generations, without leaving a single fact for history, till the time of Sardanapalus, whose vices roused the indignation of Belesys, the Babylonian, and Arbaces, the Mede. They rebelled, besieged him in Nineveh, and drove him to the desperate expedient of burning himself, his haram, and his treasures, to avoid falling into their hands. With him ended the Assyrian monarchy, according to Ctesias.

It is needless to observe that this account of Ctesias is utterly irreconcilable with the Old Testament history. It is equally devoid of probability. A series of 30 generations of kings sunk in absolute indolence, allowed to reign without molestation, is utterly incredible. Our author is of opinion that Ninus, Semiramis, and Sardanapalus, were the names of the Assyrian deities, and that the fiction originated from a practice common with the ancients of assigning the origin of nations to gods, and contriving a set of actions suitable to their conceptions of these divine kings. This opinion he supports with much ingenuity, and a great deal of learning ; and his solution of the difficulty is fully as plausible as any hitherto proposed. But I cannot see any reason for putting any confidence in the account of a writer who lived so long after the events which he attempts to describe as Ctesias, unless he had favoured us with the contemporary authorities upon which his supposed facts were founded. Now this, in the present circumstances of the case, would have been obviously impossible.

XIX. *A Descriptive Account of the several Processes which are usually pursued in the Manufacture of the Article known in Commerce by the Name of Tin-plate.* By Samuel Parkes, F.L.S. &c.—This is an interesting and entertaining paper. The author has a turn for describing manufactures, and the public are already indebted to him for several other manufactures of rather an interesting nature, of which he has drawn up a description. Indeed his Essays, if, in a subsequent edition, he would reduce them to half their present size, by omitting all the extraneous matter, would be a most valuable book, and more profitable to the author than it is likely to be under its present form.

The art of making tin-plate, or of covering plates of iron with tin, seems to have been established in Bohemia before it existed any where else in Europe. About the beginning of the 17th century, mines of tin were discovered in Saxony, and the Elector had the address to transplant the tin-plate manufactory to his own kingdom. In the year 1665, when Mr. Andrew Yarrington visited these manufactories, they were of such extent as to employ about 80,000 workmen ; and the tin-plates were sent to

all parts of the civilized world.   Mr. Yarrington went over at the
expense of some public spirited individuals to learn the art of
making tin-plate.   On his return to England many thousand
plates were made under his direction, which were considered by
all good judges as superior to the Saxon tin-plates.   But he and
his partners were prevented from establishing a manufactory by
the conduct of Charles II. who granted a patent to one of his
courtiers for making tin-plate.   This courtier did not, possess
the requisite skill, and the patent prevented those who possessed
the skill from establishing the manufacture.   The art of making
tin-plate does not seem to have been practised in England till
about 1720.   A manufactory was then established at Pontypool,
in Monmouthshire, where the art is still practised to a consider-
able extent.

  The best English bar iron (prepared with charcoal instead of
coke) is employed in this manufacture.   It is rolled at the mill
by a peculiar process into plates of the requisite thinness.
These plates are then cut by hand shears, or by machinery, to
the requisite sizes.   The plates are then bent into the form of
the Greek Λ, and put into a furnace, heated by flame from a fire-
place of a peculiar construction, after having been previously
cleansed, as it is called ; that is, steeped for four or five minutes
in a mixture of muriatic acid and water, in the proportion of four
pounds acid to three gallons water.

  In the oven they remain red-hot, standing upright on the floor
till the heat takes a scale off their surface.   They are then taken
out, allowed to cool, straightened, and beaten smooth upon a
cast-iron block.   The plates are then smoothed and polished by
passing them cold between a pair of cast-iron rollers properly
hardened and finely polished.

  These rollers are made by pouring the melted iron into a thick
cast-iron box.   The consequence is, that the surface loses its
heat rapidly, and becomes hard, while the central part of the
roller remains soft.   The art of making these *rolls*, as they are
called, is imperfect, as the process often misgives.

  The plates thus smoothed are steeped for 10 hours in *the lies*,
which is water acidulated by means of bran.   In the lies, the
plates stand on their edges, and they are turned once, which is
called working the lies.   The plates, being taken out of the lies,
are agitated for about an hour in a liquid composed of a mixture
of sulphuric acid and water.   In this mixture they become
perfectly bright, and free from the black spots which are always
on them when they are first immersed in it.

  When the plates come out of this pickle, they are put into
pure water and scoured in it, with hemp and sand to remove any
remaining oxide or rust of iron that may be still attached to them;
for wherever there is a particle of *rust*, or even of *dust*, upon
them, there the tin will not fix.   After being scoured, they are

put into fresh water and preserved for the process of tinning; for it has been found that they will not rust in pure water though kept in it for a twelvemonth.

An iron pot is nearly filled with a mixture of block and grain tin in a melted state; and a quantity of tallow or grease sufficient, when melted, to cover the fluid metal to the thickness of four inches is put to it. The tin is kept as hot as it can be without inflaming the tallow on its surface. Another pot fixed by the side of the tin pot is filled with grease, into which the plates are put, and allowed to remain an hour before they are dipped into the tin. They are then dipped into the tin pot in a vertical position: 340 plates are usually put in at once; and that they may be thoroughly tinned, they usually remain in it an hour and a half. They are then taken out, and laid upon an iron grating to allow the surplus tin to drain from them. But as too much tin always adheres, this excess is removed by a subsequent process called *washing*.

A vessel, called the *wash pot*, is nearly filled with the best grain tin in a melted state. Into this vessel the plates are put. This mass of hot metal soon melts all the loose tin on the surface of the plates, which so deteriorates the grain tin that after 60 or 70 boxes (a box is 225 plates) have been washed in it, the quantity of a block (three cwt.) is usually taken out, which is replaced by a fresh block of grain tin. When a plate is taken out, it is brushed first on one side, and then on the other, by a brush of hemp of a peculiar kind. It is then dipped once more into the wash-pot. It is then plunged into a pot filled with melted tallow, the temperature of which is carefully adjusted to the thickness of the plates. Here the excess of tin runs off, and is collected in a wire at the bottom of the plate. From the grease-pot the plates are removed into another empty pot to cool, and when they are cold enough to handle, a boy, called the *list* boy, takes them and dips them one by one into a pot containing a little melted tin in its bottom. When the wire of tin is melted, the boy takes out the plate, and gives it a smart blow with a thin stick, which disengages the wire of superfluous metal, and this falling off leaves only a faint stripe in the place where it was attached. Nothing now remains but to clean the plates from tallow. This is done by means of bran. They are then put into wooden or iron boxes made exactly to fit them, and are ready for sale.

XX. *The Laws of Statical Equilibrium analytically investigated*. By Mr. John Gough.—This ingenious and valuable paper is of a nature not to admit of abridgment. I am, therefore, under the necessity of referring the reader to the volume itself, assuring him that it will repay his trouble if he will give it a careful perusal.

XXI. *Experiments on the Gas from Coal, chiefly with a View to its practical Application*. By William Henry, M.D. F.R.S. &c.

—This paper is already printed in the *Annals of Philosophy*, vol. xiv. p. 344.

XXII. *An Inquiry into the Effects produced upon Society by the Poor Laws.* By John Kennedy, Esq.—The object of this ingenious paper is to defend the system of the poor laws as established in England. The author ascribes to their agency the spirit of independence, the superior comforts, and the uncommon industry, which distinguish the common people in England. He thinks that if we take into view the difference in the value of money and the great increase of the population, the poor rates are not higher in England at present than they were 60 years ago. He, therefore, conceives, that the poor rates, instead of being injurious to England, have in reality been the means of creating a spirit of independence and industry, to which the country has been indebted for its prosperity and power. I am very willing to give the author full credit for the benevolence of his views, and the goodness of his intentions; but conceive that his mode of estimating the advantages resulting from the poor laws is in a great measure fallacious. The science of political economy is much more complicated than any other. Hence it is much more difficult to draw the proper consequences from facts in that science than in any other. That England, during the last century, made prodigious advances in wealth and power is admitted by every one. The question is to what are we to ascribe these advances? Some have ascribed them to the freedom which the inhabitants of this island enjoyed; some to the successful wars which were carried on in the course of the century, which enabled England to cripple, and even annihilate, some of the most lucrative branches of trade and manufactures carried on by our rivals; some to the erroneous policy and misgovernment of the other European states; some have even ascribed the progress of England to the monopolizing laws established by the influence of our manufacturers; and some have been bold enough to ascribe it to the increase of taxation; but our author, so far as I know, is the first person who has ascribed it to the influence of the poor rates.

He allows that the present system of poor laws is liable to abuses that cannot be easily checked. This alone is an objection, which appears to me decisive. He thinks the poor in England are better fed, and enjoy much greater comforts, than the poor in Scotland. If by *poor* be meant those persons who are supported by the parish, the statement, for any thing that I know to the contrary, may be true; but if by *poor* be meant, according to the usual acceptation of the term, the class of people who are obliged to labour for their maintenance, I have some reasons for doubting the accuracy of the assertion. The expense of living is not greater in Scotland than in England, and yet the wages of farmers' servants is considerably higher in Scotland than in England. A Scotch farmer's servant then has the

means of living better than the same class of persons in England. It is true that his style of living is different. He does not indulge in the large draughts of beer which an English hind cannot do without, and which in reality constitutes the greatest part of his food; but he has a liberal supply of milk, which, in my opinion at least, is a far better beverage. He is not in the habit of wasting the whole of his income upon his immediate subsistence. He endeavours to lay up a little for old age, and he has frequently an old father or mother whose support has devolved upon him. Now I leave our author to determine which of these two modes of living is likely to foster the most industrious habits and the most independence of spirit. In the south of England at least, for I have not had the means of witnessing what is the practice in the north of England, the wages of the farm servants are so low that the deficiency is partly made up by the parish, and farmers are not in the habit of hiring them by the year but for very short periods. Now if living upon the parish be the way to acquire independence of mind and a sense of one's own dignity, it must be acknowledged at least that the medicine in general is very unpalatable.

Our author says that the poor rates lower the wages of labourers, and enable the manufacturers to produce their cotton and their calicoes at a cheaper rate than they otherwise could do. Of course, says he, it is the interest of the manufacturers to uphold the poor rates. Now as far as the manufacturers are concerned there is something in this argument. When turned into plain English, it will stand thus: Part of the wages of manufacturing labourers is paid by the country in general (by manufacturers, landed proprietors, farmers, &c.) whereas, if there were no poor rates, they would be all paid by the manufacturers themselves. Therefore it is for the interest of the manufacturer that the poor rates should continue; so that the poor rates act as a bounty on manufactures. So far they are really injurious to the country by enabling manufacturers to persist in making several articles after they have ceased to be profitable. In reality, as far as the labourers of manufacturers are paid by means of the poor rates, the money so bestowed is given away to those foreign countries who purchase our manufactures; and England would be just as much richer as she is by the amount of the poor rates thus given to the labouring manufacturers, if these manufactures were not cultivated in the country at all.

XXIII. *Memoir on Sulphuric Ether.* By John Dalton.

XXIV. *Observations on the Barometer, Thermometer, and Rain, at Manchester, from* 1794 *to* 1818 *inclusive.* By John Dalton.

The first of these articles was given entire in our last number; the second is reserved for our next.

ARTICLE VIII.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

Dec. 3.—A paper was read by the Rev. Professor Buckland, of Oxford, On the Quartz Rock of the Lickey Hill, near Broms-grove, and Strata immediately surrounding it, with Considera-tions on the Origin of Quartzose Pebbles of the Plains of Warwickshire, and of the Valley of the Thames from Oxford downwards to London.

The group of the Lickey Hills is described in this paper as occupying a small district between Bromsgrove and Birming-ham in the middle of an extensive tract of young red sandstone.

At the Lower Lickey this sandstone suddenly ceases, and a long hill composed of granulated quartz rock projects to a consi-derable elevation above the plains of sandstone that flank it on the east, forming a narrow camel-backed ridge of about two miles in length, from north to south, and a quarter of a mile in breadth. On each side of this ridge is a small deposit of shat-tered strata, belonging to the coal formation, and near the extreme points of its north base are two minute patches of tran-sition limestone. At its south end there is also a small portion of trap rock, and near its north-east base there occur traces of cornstone and old red sandstone. These fragments, together with the ridge of quartz rock, are encircled by an investiture of horizontal strata of young red sandstone, the beds of all the other formations being highly inclined.

The upper Lickey Ridge, which overhangs all these fragments of older formations on the west, contains a higher elevation than any of them, and is composed of strata belonging to the young red sandstone formation, containing subordinate beds of pebbles derivative from the quartz rock.

The ridge of quartz rock constitutes the most important feature of this group, and is probably referable to a place among the oldest members of the transition series, being exactly of the same character with the quartz of the summit of the Stiper stones, and the Wrekin and Caer Caradoc, in which two latter places, it lies beneath greywacke slate, and is incumbent on greenstone.

The quartz at the Lickey is distinctly stratified, and is natu-rally shivered or split into millions of small angular fragments; this circumstance is important, as showing the facility with which a rock so constituted, may have been broken down by the force of water, and have contributed to form those enormous beds of siliceous pebbles which occur in the lower strata of the young red sandstone formation in the midland counties of Eng-

land. Other pebbles that are mixed with them in the same strata are probably derivative from the rocks of Charnwood Forest. The destructibility of this rock, in consequence of its shattered and minutely divided state, affords a reason why those few portions of it which we find in the Lickey and at Caer Caradoc and the Wrekin, are almost the only remaining fragments of a formation which the abundance of its wreck proves to have occupied a very considerable space before the deposition of the strata composing the young red sandstone formation. Several places are specified in the neighbourhood of Bridgnorth and Kidderminster where these siliceous pebbles may be seen forming part of, and imbedded in, the regular strata of the young red sandstone formation.

In the same neighbourhood, and indeed universally over the central plains of England, the same pebbles occur mixed with other pebbles of almost every kind of English rock from granite up to chalk, in the form of superficial gravel torn by the last diluvian waters that have affected the earth from every substance that lay within the influence of their currents, and drifted together without any reference to the age or condition of the subjacent strata on which they are now accumulated.

The extent of this gravel is not limited to the central plains; it has been drifted on within the area of the oolite formation by two depressions or low lips in the high escarpment of the Cotswold hills, and has passed down the Even-lode and Charwell towards Oxford.

The table-lands on each side the valley of Even-lode are scattered over with those pebbles, and on the highest summit of Witchwood Forest, Wytham Hill, and Bagley Wood, on the north-west of Oxford, thick lips of gravel composed of these pebbles from Warwickshire, are accumulated on the surface of strata belonging to the oolite formation.

The position of these summits is exactly opposite the point where the valley of the Even-lode falls into that of the Thames, and where the diluvian currents descending the former valley would evacuate their driftings.

Similar pebbles occur in the gravel of the summit of Henley Hill and in the gravel beds of the valley of the Thames, from Oxford downwards to Hyde Park.

But above that point where the driftings of Even-lode Gorge fall into the valley of the Thames, no such deposits of siliceous pebbles are to be found along the course of the Salter river from its head springs on the south side of the Cotswold Hills. There is a considerable proportion of chalk flints mixed up with the gravel we are considering; and the fact of its containing pebbles of hard white chalk and of red chalk, such as occurs in no part of the chalk of the south-east of England, but is common in the lower strata of this formation, in Lincolnshire and Yorkshire, goes far to show that these red pebbles and chalk flints have

been drifted south-westward, probably from Lincolnshire, over
the plains of Leicestershire, subjacent to the great oolite
escarpment. It is certain the quartzose pebbles cannot have
been drifted from any part of England south-east of the escarp-
ment, since there is no stratum in this portion of the island from
which they could possibly have been derived. It is stated that
these quartzose pebbles, which were derived primarily from the
Lickey quartz rock, received their rounded form at a period pre-
ceding the deposition of the young red sandstone strata, from
which they were again torn up and mixed with fragments of
other rocks, and scattered over the surface of all formations that
lay in the course of the latest diluvian currents that have affected
the earth. The completely rounded flint pebbles of Blackheath
and of the Hertfordshire pudding-stone are also stated to have
received their attrition at a period intermediate between the
deposition of the chalk and plastic clay formations, and long
anterior to the action of the last great diluvian waters.

And both these pebble beds of ancient origin have shared the
common fate of all formations in being torn up by the waters of
the last great deluge, and mixed up with the less perfectly rolled
fragments of rocks that have undergone no further attrition than
that to which they were submitted in being washed by it from
their native station to the gravel beds they now occupy.

An examination of the compound character of the gravel beds
of Kensington and the valley of Oxford is brought forward in
proof of the statements above advanced.

At Kensington, we have the ancient pebbles which existed as
such in the regular strata of Warwickshire and Blackheath
mixed up with the angular and imperfectly rolled chalk flints
which constitute the mass of that gravel, and have been sub-
mitted only to the action of the last great deluge.

And at Oxford we have the same Warwickshire pebbles mixed
with the angular and slightly rolled fragments of oolite and other
rocks which form the hills intermediate between Warwickshire
and Oxford. Arguments are also adduced to show that the
lower trunks of the valleys of the Thames and Even-lode, i. e.
those portions of them which lie between the table lands that
flank the course, did not exist at the time of the first advance of
the diluvian waters which brought the pebbles from Warwick-
shire, but were excavated by the denuding agency which they
exercised during the period of their retreat.

Dec. 17.—The conclusion of a paper was read, which had
been begun last session, " On the Coal Fields adjacent to the
Severn," by Prof. Buckland and the Rev. W. D. Conybeare.

Of these coal fields the first selected for description is that
which they have termed the Somerset and South Gloster Basin.
It is called a basin only with reference to the general position of
the strata, comprising its interior, since these strata are affected
by many minor undulations which at first sight appear to divide

it into several smaller basins and irregular concavities which may be considered subordinate to the general structure of the district.

This district occupies a triangular area bounded on the south by the Mendip hills, and on the other two sides by lines drawn from the two extremities of the Mendip chain to the village of Tortworth, in Gloucestershire.

The exterior ridges which bound the area of this coal formation consist for the most part of an interrupted chain of mountain limestone reposing on old red sandstone ; and these two formations are so closely associated, that in describing their geographical extent, it is necessary to treat of them in combination.

Of these extensive ridges, the Mendip chain is most important in extent and elevation ; it consists of a central axis of old red sandstone, flanked by a double line of mountain limestone ; the sandstone strata, however, are not visible in one continuous line ; but present themselves in a series of four ridges, forming the four most elevated parts of the chain, though not exclusively confined to them.

Each of these ridges forms a saddle-shaped nucleus of sandstone, usually inclined at an angle exceeding 45°, around which the incumbent beds of limestone are wrapped as a mantle dipping in every direction, conformably to the subjacent sandstone.

Between the sandstone and mountain lime is interposed a bed of shale, in which several fruitless attempts have been made in search for coal : this bed it is proposed to designate as the lower limestone shale. The Mendip chain is thus divided into four regions, each containing as its nucleus one of the ridges of old red sandstone.

The most easterly of these is the ridge of Masberry Castle, near Shepton Mallet ; the east central is Pen Hill, near Wells ; the west central, Priddy North Hill and the western Blackdown, near Cross.

The total length of the Mendip chain is about 26 miles running east and west from near Frome to Uphill, on the Severn ; on the south-east flank of this chain a curious circumstance is exhibited in the contact of overlying horizontal beds of lias and inferior oolite, with the inclined strata of mountain lime and old red sandstone, several small rivers in this part exhibiting romantic cliffs of mountain lime in highly inclined strata, crowned with horizontal beds of oolite.

These strata sometimes adhere to each other as firmly as if they had been parts of one and the same contemporaneous formation.

The detail of the description of the subdivisional ridges of the Mendip chain is too minute to allow of abridgment.

The mountain limestone corresponds in all its characters with that of Clifton, and has its usual properties of engulphing rivers, and exhibiting extensive caverns and abrupt romantic precipices, the most remarkable of which are at Chedder cliffs. The old

red sand throughout this chain is characterized by a degree of sterility as remarkable as that of its fertility in the countries of Monmouth and Hereford : this arises from its being composed in Mendip of a hungry, siliceous grit, alternating with beds of brown tenacious clay ; and being deficient in those beds of marl coloured by red oxide of iron, which give their extraordinary fertility to the last two named counties, and which extend their influence to the similarly constituted strata of the young red sandstone and red rock marl formation.

Jan. 7, 1820.—The continuation of a paper was read, " On the Coal Fields adjacent to the Severn," by Prof. Buckland and Rev. W. D. Conybeare.

The southern boundary of the triangular area of the Somerset and South Glouce'ster coal field, having been described as constituted by the entire chain of the Mendip hills, it remains to examine its west and eastern border, and afterwards to proceed to the history of the coal field itself.

On the west border, the old red sandstone and mountain limestone do not, as in the Mendip chain, form a continuous unbroken frontier, but occur in the detached groups of Brondfield Down, Leigh Down, Derdham Down, Kings Weston Down, and the ridgway near Almondsbury, extending thence to Tortworth, which is situated on the north apex of the area under consideration.

The strata of mountain lime composing Brondfield Down dip in every direction from the centre to the circumference, as is expressed on the map by arrows and figures ; its south hangings are covered by beds of calcareous conglomerate, some of which are crowned with caps of lias.

The west side of Brondfield Down, being also the steepest, is intersected by three magnificent gorges, resembling Chedder Cliffs ; of these Brockley Comb and Gobble Comb are the most important ; the latter has a strong resemblance to Dovedale, in Derbyshire.

In a trough of limestone between Brondfield Down and Leigh Down is situated the small coal field of Nailsea, which is a kind of satellite exterior to, and attendant on, the great coal basin, to the edge of which it is externally adjacent.

The chain of Leigh Down extends from Clevedon on the Severn to the gorge of the Avon, near Clifton ; it dips south, and has beds of old red sandstone forming the base of the escarpment of its east portion ; nearly parallel to this chain on its north-west side is a similarly constructed ridge called Weston Down, having a similar dip ; and in the valley between them are the shattered strata of the coal field of Clapton reposing on the limestone of Weston Down, and touching the base of the escarpment of the west portion of Leigh Down ; an extensive fault occurs along the base of this portion of Leigh Down, and brings the coal measures into contact with the basset edges of the limestone.

The base of Leigh and Weston Down, and the valley situated between them and the Avon, are, for the most part, covered with horizontal strata of magnesian conglomerate.

The west boundary of the main coal field is continued on the other side of the gorge of the Avon in the calcareous chain that winds round the south-east and north sides of the vale of Westbury, and constitutes Durdham Down, Henbury, and Kings Weston Down. The fundamental part of this valley is old red sandstone, the truncated edges of which are, for the most part, covered by horizontal beds of magnesian conglomerate; the dip of the old red sandstone and limestone is outwards in every direction from the axis of this valley.

The next group of mountain lime and old red sandstone along the west frontiers of the main coal field is that which extends from the ridgway near Almondsbury to the village of Tortworth, and which dips regularly towards the interior of the coal basin with a few partial exceptions.

The intermediate space between Almondsbury and the Westbury group shows only beds of the lias and young red sandstone formation, which on this side intrude largely within the coal basin, extending to Stoke Park, on the edge of the Kingswood collieries, and, in all probability, covers up considerable tracts of coal; this lias seems to have been once connected with that of Pucklechurch and Sodbury, covering the whole South Gloucester coal, from which it has been since partially removed by denudations.

At Tortworth the calcareous chain of the west frontier reaches the point where it is suddenly deflected southwards at an acute angle, and from which it may be traced with certain interruptions along the east frontier of the great coal basin to the Mendip hills, near Mells.

Near Tortworth, beds of transition lime are protruded from beneath the old red sandstone, and are intersected by two dykes of trap. We also find there a few traces of coarse greywacke slate.

ROYAL ACADEMY OF SCIENCES AT PARIS.

An Analysis of the Labours of the Royal Academy of Sciences during the Year 1818.

(*Continued from p. 65.*)

REPORTS APPROVED OF BY THE ACADEMY.

Experimental Researches upon Lime for Building; by M. Vicat.—As the whole of this important report will be printed in an early volume of the Memoirs, we shall now only transcribe the conclusion of the report of the committee, Messrs. de Prony, Gay-Lussac, and Girard, Secretary.

" This work engaged the attention of your Committee both on account of the new facts it contains, and the order and clearness

with which they are related. The explanations given of them
are founded on sound theory, and prove that the author, although
he resides in a distant department, has yet constantly kept pace
with the progress of the sciences, and is very capable of deduc-
ing useful applications from them. No one can fail to conduce
to this progress if, with such a spirit of inquiry as appears to
animate M. Vicat, he endeavours to enlighten with his know-
ledge the art in which he is employed, and engineers who are
placed in similar circumstances in different parts of the kingdom,
are under obligations to him both for the results of the work he
offers to them, and the example he offers them." We think,
therefore, that this work does in every respect merit insertion in
the Recueil des Savans Etrangers.

Euclid in Greek, Latin, and French; by M. Peyrard.—Com-
mittee, Messrs. de Laplace, Legendre, Prony; and Delambre,
Secretary.

This third and last volume contains the 11th, 12th, and 13th
books of the Elements, the book of the *data;* and, lastly, the
two supplementary books on the *regular solids* which are not
really by Euclid, and are generally attributed to Hypsicles of
Alexandria. The editor has thought it right to apologize for
having republished these two books, which he does not appear
to esteem very highly ; he is in our opinion not only sufficiently
justified by the example of so many other editors, one of whom
even thought it right to add a new supplement to those of Hyp-
sicles ; but we may say that these books are a necessary conti-
nuation of the 13th book of Euclid, who had merely touched
upon the theory of regular solids. In fact, Euclid contented
himself with settling the edges of these bodies without saying a
word about the mutual inclinations of their surfaces, or the
distance of these surfaces from their poles, or from the centre of
the sphere, nor does he speak of the surfaces or of the bulks of
the five regular solids.

Hypsicles has not, however, entirely exhausted the subject,
he merely gives the surfaces of the dodecahedron and the icosa-
hedron, he determines their proportions, which is also that of
their bulk, since the surfaces of these two bodies are at equal
distances from the centre of the sphere—a remark which he
might have extended to the hexahedron and the octahedron, as
has been done by one of his continuators.

The subject of the inclinations is treated of more fully. In
order to determine them, Hypsicles begins by explaining the
general construction of his celebrated master, Isidorus. The
matter appeared so evident to that geometrician that he thought
it unnecessary to add any demonstrations. At first sight, it
might be imagined that Hypsicles wished to render it obscure
while demonstrating it, but from every appearance Isidorus
had, when he invented his constructions, the figures in relief of
all the regular solids. With this assistance, which M. Peyrard

also procured, nothing is required but the use of our eyes to be convinced of the perfect accuracy of these methods; we may then easily comprehend the figures traced by Hypsicles, and the demonstrations become clear. M. Peyrard accuses these demonstrations of being deficient, both in exactness and in elegance. We allow they are much too long, but that fault ought not to be thrown on Euclid, who, we know not why, has determined the inclination to be the acute angle formed by two contiguous surfaces. In reality the inclination is never an acute angle, except in the tetrahedron : it is a right angle in the hexahedron, and an obtuse angle in the other three solids, so that an acute angle is not to be found in the hexahedron, and in the three other solids, it is the angles between one surface and the prolongation of the neighbouring surfaces that are acute. Now half the demonstrations of Hypsicles are devoted to settling the species of angles, whilst the constructions of Isidorus always give the true angle, whether acute, or obtuse, so as to preclude the possibility of any mistake.

We may add that these demonstrations, although they are different for each of the five regular solids, yet depend upon *one* single principle, which would render them clear, even independently of the figure in relief. This principle consists in supposing in each solid a line to be drawn, which would serve as a common basis to two isosecles triangles, whose sides are known. In one of these triangles the angle at the summit is always known; in the other, it is the inclination which we seek for; a very simple relation between the cosines of the two angles is the result of this ; and if we apply to these triangles, one of the rules of our modern trigonometry, we immediately obtain an equation exactly similar to that which is furnished by spherical trigonometry.

But this modern rule was entirely unknown to Euclid, to Isidorus, and to Hypsicles, who, in the very defective solution which he has elsewhere given us of a problem resolved nearly at the same time by Hipparchus, has left us a striking proof of his complete ignorance of both plane and spherical trigonometry.

It is rather remarkable that this theory of regular solids, perplexed and imperfect as it was with the Greeks and their continuators, should depend entirely upon a rectangular spherical triangle, traced on the surface of the sphere upon which we wish to inscribe at once all these solids. The angles of this triangle are always given, and the formulæ resulting from them for the three sides furnish the most simple expressions of the edges of the polar distances of all these planes of their mutual inclinations, of their distances from the centre of the sphere ; and, lastly, of the methods of measuring with equal facility the surfaces either partial or total, and the bulks of the five regular solids in parts of the radius of the sphere taken as unity.

"This triangle not only gives the precise and numerical quantity

of the inclinations, which was beyond the geometry of Euclid, but it also supplies the most simple relation to determine the nature and the number of the regular solids which can be inscribed in the same sphere; so that a single triangle, a single formula, serves for all. This will be demonstrated by one of us in the ' History of Modern Astronomy,' at the Article Kepler, who wished to prove, by means of the five regular solids, that no other planets than those known from time immemorial could exist.

" Another observation, not less curious or new, is, that the general trigonometrical expressions (the most expeditious that can be imagined for logarithmic calculation) may be transformed with wonderful facility into those irrational expressions which the Greeks call *major, minor,* and *apotome.* In fact all the primitive angles are of 30°, 36°, 45°, 54°, 60°, and 90°, whose trigonometrical lines are irrational, and lead directly to the constructions of Euclid and of Isidorus. The consequence of this is, that the unknown parts of each problem may be expressed at our option by the sines, cosines, and tangents, either of the arc or of its half, so that we have always six different expressions for every one, and among so many expressions, we may always select the most convenient; the calculation is also shortened still more by the circumstance that there is scarcely one of these quantities which is not to be found again in another of these solids ; so that there are never more than four calculations in the whole to make for the 15 unknown quantities of the general problem. In this way, after having completed and simplified the constructions of Euclid for the five edges, we have succeeded in forming more easy and uniform constructions than those of Isidorus, in rectilinear isosceles triangles, whose common basis is the diameter of the sphere.

" We, therefore, think we may differ in opinion from the translator, and consider the two Books of Hypsicles as a curious remnant of the ancient geometry, inasmuch as they contain notions that are not to be found elsewhere. The most important points are, to obtain true theorems, and faultless constructions. As for the demonstrations, they likewise are of importance, doubtless; but should we be dissatisfied with them, we may, without much difficulty, find others. The principal defect of those of Hypsicles has already been mentioned; it is, that the first half is in every one of them quite useless.

" It is true that the demonstration of the second proposition in the second Book was perfectly unintelligible ; but we may be allowed to conjecture that only the copyists are to blame. M. Peyrard has given a new one, which may probably have been originally the author's. There is also a demonstration of Euclid which all commentators had agreed to look upon as altered, or as quite deficient. It is one of the most important propositions in the book of data, and may be reduced to an

equation of the fourth degree, which is solved in the same way as those of the second. M. Peyrard begins by giving the algebraic calculation of it; afterwards, by translating the Greek demonstration into the modern style, he renders its strict and accurate progress more forcibly evident.

"Amongst all the propositions contained in this last volume, not one occasioned the editor so much trouble as the 17th of the 12th Book of the Elements. In all manuscripts and editions whatever, the figure was so defective, that most of the reasonings of Euclid were quite inapplicable to it. The translator has, by means of adding some lines, rendered the demonstration exact in every respect.

"There is, in every other part, as in the preceding volumes, the most perfect fidelity in the translation, and the same care is taken to correct the text, and to collect the various readings, which here form 84 pages. The editor had asserted that the fine Oxford edition was not more correct than that of Basil, since, besides republishing all the errors, even the most palpable, of the latter, the former contained a considerable number of other faults, from which the Basil edition was free. This statement excited astonishment, and was probably but little credited; yet we cannot perceive what can be objected to the eight pages in which M. Peyrard has exhibited a comparative view of the two editions.

"M. Peyrard has now brought a long and laborious work to a successful conclusion. We propose to the Academy to extend the same approbation it has been pleased to bestow on the two others, to the third volume, in the hopes that this merited approbation may facilitate the publication of the author's Apollonius, the manuscript of which has long since been finished."

We have much pleasure in announcing that this new edition is begun, and that we have already seen several sheets of it.

A Treatise on Descriptive Geometry; by M. Vallée, Civil Engineer.—Committee, Messrs. de Prony, Fourier, and Arago, Secretary.

The lectures on descriptive geometry of the celebrated author of that doctrine, M. Monge, contain an explanation of the principles of the science which will always be cited as a perfect model of perspicuity. It is to be regretted that the work is not more extensive, for artists who have not made a particular study of mathematics cannot familiarize themselves with the methods of projection, without varying the data of the questions, and practising upon a great number of examples. M. Hachette has partly filled up this chasm by a supplement, which obtained the approbation of the Academy. By following the footsteps of these two scientific men, M. Vallée has compiled his treatise, which he divides into six books, forming more than 500 pages in quarto. "The Committee, who devoted their attention chiefly to the most difficult parts, feel pleasure in

acknowledging that they are composed with much order and clearness. The 59 plates accompanying the text are perfectly well drawn. Each diagram presents even to the minutest particular every construction that it is necessary to execute, in order to obtain the solution of the problem, and yet there is not the least confusion. In a word, this work appears to us, in every respect, worthy the approbation of the Academy. We wish this skilful engineer may receive from government such encouragement as will enable him to publish his work, and also that he may finish those he has already undertaken, and which are to contain the applications of descriptive geometry to the arts of the carpenter and of the stone-cutter."

A Treatise on Surveying; by M. Puissant. Second Edition. —The first edition of this work having been speedily exhausted, the author has, in preparing the second, enriched it by some important additions. *La Base du Systéme Métrique, la Meca- nique Celeste,* and the Memoirs of M. Legendre, are the fruitful mines from whence he has frequently drawn. It would, how- ever, be wrong to suppose that, even in those cases, he has merely acted the part of a copyist, the new and elegant demon- strations which he gives of the already known formulæ, and the connexion he has established between theories, that had often been presented separately and by different geometricians, prove that M. Puissant had, before he took up the pen, deeply studied the methods of geodesy. The Committee are of opinion that the new work of this skilful engineer deserves, in every respect, the approbation of the Academy.

Model of a Machine for raising Water by the combined Action of the Weight of the Atmosphere on the Surface of the lower Reservoir, and the Reflux of that Water in an ascending Pipe, inserted in a Kind of intermediate Reservoir, filled by Means of the Vacuum occasioned in it by the said Mechanism; by Messrs. Lacroix and Peulvay.—Committee, Messrs. de Prony, Charles, and Girard, Secretary.

The Committee begin by explaining by what means the want of the usual pistons, valves, and suckers, have been supplied. From the description they afterwards give of all the parts of the machine, and the methods of bringing it into action, they con- clude it may be reduced to a sort of wheel furnished with a certain number of wings, capable of opening as they turn round to form successively as many partitions. The idea of this sort of pump appears to them very analogous to a plan that Conté car- ried into execution 12 years before his departure for Egypt. They even think the machine of Conté was rather more simple; the new model nevertheless proves the artists to be ingenious and intelligent. If the invention be not as new as they seem persuaded of, yet it cannot be denied that their sucking and forcing pump may, in certain cases, be advantageously substi- tuted for the common pumps, and that the authors have

displayed talents that deserve encouragement. They add, that in the Description des Machines de Serviere, there may be seen an account of an apparatus constructed in the house of M. Lenoir, Faubourg Saint Antoine, in which may easily be perceived a close resemblance to the machines of Contè, and Messrs. Lacroix and Peulvay.

Notice relative to Iron Railways; by M. Gallois.—Committee, Messrs. de Prony and Girard, Secretary.

It has long been the custom in some parts of Germany to make, for the purpose of working mines, roads, or carriageways, formed of longitudinal pieces of wood, on which were drawn, through the galleries, carts, appropriated to that sort of convey-ance. Since then a plan has been invented in England of substituting for these pieces of wood, roads, or ways, made of cast iron. M. Gallois has undertaken to describe them more fully than has hitherto been done, and to estimate their advan-tages as compared with those of common roads and navigable canals.

It is in the neighbourhood of Newcastle that iron railways are particularly numerous. According to the author, an extent of 28 square miles on the surface of the earth presents a series of 75 miles of them; while the interior of the coal mines contains them to as large an amount. Five or six waggons, made entirely of iron, fastened to each other in regular succession, descend these roads without any other mover than their own weight. By means of a pulley, or wheel, a certain number of carriages in descending occasion a certain number of others to mount, in order to unload, or to take in a load at the summit of the inclined plane they traverse. The principal object M. Gallois appears to have in view consists in displaying the advantages the iron railways, so frequently to be met with in England, have over common roads and navigable canals. We are naturally led to believe that, excepting in very peculiar circumstances, there will always be a great saving of power in conveyances by water, for this simple reason, that the whole weight of the burden so transported is supported by the stream, while the inclined plane upon which the carriage runs supports only a part of its weight. On the other hand, it cannot be denied that many situations in which it would be quite impossible to open a canal, might admit of the establishment of railways of wood or of iron. To diffuse the knowledge of their construction is supplying our industry with new sources of employment. But previously to their employment, it would be necessary to render the casting of iron less expensive. This important object has particularly fixed the attention of M. Le Gallois. His notice on iron railways is one of the most useful results of the researches in which he has been engaged during his stay in England; and, in our opinion, the Academy ought to encourage its publication.

Methods of cutting out Garments; one by M. Beck, tailor at

Paris; the other, by M. Chomereau, tailor at Brie-Comte-Robert.—Committee, Messrs. de Prony and Molard, Secretary.

The following is the conclusion of the report:

" It is possible that Messrs. Beck and Chomereau may have been anticipated on some points by those who have treated of the same subject before them, but we are not the less obliged to them for their efforts to submit the art of the tailor to rules that tend to render perfection and economy compatible. We think their zeal and their talents deserve praise from the Academy."

Essay upon a general Principle in Mechanics; by M. Binet.—Committee, Messrs. De Laplace, Poisson, and Fourier, Sec.

" The author principally considers the proportion between the powers and the areas described by the radii vectores, round a fixed centre. He denominates the fluxion of the area traced by the radius vector the areolar velocity, to distinguish it from the linear velocity, which the moving body actually describes in its trajectory line. He calls the sum of the products of the masses by the squares of these areolar powers the areolar *vis viva*, and determines the mathematical relation of these quantities. If the power that acts upon a moveable point be represented by a right line given in magnitude and position, and there be drawn upon this line, as on a base, a triangle, whose summit represents the fixed centre, this figure will represent the force of rotation, the plane of the triangle being that in which it exercises its action. If the moveable body passes from the place it occupies into another infinitely near, its radius vector will describe an infinitely small area, the plane of which may differ from that of the rotary power. If on this last plane the area described is projected, the projection will represent the virtual effect of the force of rotation estimated in the very plane of that force. This being done, it may be enunciated as if it were the principal result which the author attained.

" If the quantity of each force of rotation be multiplied by its virtual effect, and if all the similar products be added, the sum will represent the instantaneous increase of the whole *vis viva*, relatively to the areas described, or the sum of the products of each mass by the square of the velocity by which the area was increased. By thus determining the element of the total *vis viva* for every instant which follows, and by adding those elements together, the integral will express the increase that the *vis viva* receives during any given time.

" This proposition is entirely similar to that which expresses the linear *vis viva*. The same analysis that shows what these propositions have in common, shows also in what manner they differ. Kepler, to whom we owe the discovery of the elliptic motions of the planets, found at the same time, by an assiduous comparison of observations, that the radius vector of a planet described areas proportional to the times. Newton afterwards ascended from the knowledge of the mathematical laws deduced

from observations to that of the physical cause of the phenomena. He saw that this equality of the areas necessarily shows the power that retains the planet in its orbit to be directed towards the sun. Thus every one of Kepler's laws became a theorem in dynamics. D'Arcy, Bernouilli, and Euler, well knew that if the areas were drawn upon any plane, the sum of the areas, measured in one direction, would augment in proportion to the time elapsed."

Here the Committee mention in an abridged manner the theorem of Newton relating to the conservation of the centre of gravity; that of M. De Laplace concerning the plane of the maximum of the areas, the researches of Euler concerning the measure and composition of the momenta. " The propositions relative to this composition, and many new theorems concerning the same subject, are expressed in the clearest and the most elegant manner in the Treatise, and the Memoirs which M. Poinsot has published upon Statics. Every one of these results are to be found in them, deduced by a plain method peculiar to him, which has the advantage of rendering them easy to be perceived, and of proving, in a direct manner, that the forces of rotation are decomposed, distributed, and destroyed, according to rules entirely similar to those which concur in the forces of translation. The exposition of the properties relative to areas, which Laplace has given in his Mecanique Analytique, ought to be added to this enumeration.

" M. Binet's method consists in deducing from the differential equations of the motion, the expressions relative to the areas produced, and to their fluxions of the first and second order, by proving that the expressions combine among themselves in the same manner as those of the arcs described by the moving body, and those of the velocities. This analogy between the areas and the trajectories may be considered in another point of view; in reality, if we suppose that, in the general equation which shows the sum of the projected areas to increase in proportion to the times, the centre of the radii vectores is placed at an infinite distance from the origin of the coordinates, we shall see directly that the velocity with which the sum of the areas increase is the velocity with which the centre of gravity of the system departs from a fixed plane. And in this manner the theorem concerning the motion of the centre of gravity is deducible from that of the preservation of the areas. The case is the same in regard to the equation which expresses the three parts of the *vis viva* of rotation.

" The latter end of the memoir presents an ingenious collection of several general theorems of mechanics. In order to show that these propositions arise from one common source, the author adds to them the differential equations of the motions, multiplied by the coefficients which the variable quantities may contain, and their differentials of the first order. He proposes to

determine these coefficients so that integrable expressions may be obtained. By this means a general result is obtained, which produces the theorem relative to the motion of the centre of gravity, that of the areas, that of the *vis viva*, and, lastly, the one which the author has demonstrated."

" The researches which tend to bring general mechanics to perfection are interesting not only to the arts, but also to the study of nature. It is upon these accounts that the Committee judge the work of M. Binet worthy of the approbation of the Academy ; as well for the choice of the subject, as for the manner in which the subject is treated ; and propose the printing of the memoir in the collection of foreign papers."

ARTICLE IX.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Protoxide and Deutoxide of Azote.*

IT seems to have been proved in a satisfactory manner by Gay-Lussac, that deutoxide of azote or nitrous gas is a compound of 1 volume of oxygen gas + 1 volume of azotic gas united together without any change of bulk ; so that the specific gravity of deutoxide of azote must be the mean of that of oxygen gas and that of azotic gas.

Specific gravity of oxygen gas ...... 1·1111
Specific gravity of azotic gas. ...... 0·9722
_____
2) 2·0833
_____

Specific gravity of deutoxide of azote $= 1·0416$

A set of careful experiments made in my laboratory gave 1·043 as the specific gravity of this gas. Now this comes within $\frac{1}{1000}$th part of the theoretical number—a coincidence which I consider as sufficiently near to satisfy us of the truth of Gay-Lussac's hypothesis.

From the most careful experiments hitherto made upon the protoxide of azote, there is reason to consider it as composed of one volume of azotic gas, and half a volume of oxygen gas, condensed into one volume ; for this gas is completely decomposed when mixed with its own volume of hydrogen gas, and an electrical spark passed through the mixture. There remains after the combustion a quantity of azotic gas exactly equal to the original bulk of the protoxide of azote employed. Let the *volumes of the two gases* burned be as follows :

> Protoxide of azote. . . . . 100 volumes
> Hydrogen 100

After the combustion there will remain a quantity of azote making 100 volumes. Now 100 volumes of hydrogen for complete combustion require 50 volumes of oxygen. From this it is obvious that 100 volumes of protoxide of azote are composed of

> Azotic gas. . . . . 100 volumes
> Oxygen gas 50

condensed into 100 volumes; or, which is the same thing, of one volume of azote and half a volume of oxygen condensed into one volume. Hence we must obtain its specific gravity by adding half the specific gravity of oxygen gas to the specific gravity of azotic gas.

> Specific gravity of azote. . . . . 0·9722
> Half specific gravity of oxygen . . . . . . 0·5555

> Specific gravity of protoxide of azote = 1·5277

Now Colin found the specific gravity of this gas 1·5204, and a careful set of experiments made in my laboratory gave the specific gravity of this gas 1·5269—a number which is much less than $\frac{1}{1000}$th part from the theoretical number. There seems then no reason to doubt the accuracy of the opinion, that this gas is a compound of a volume of azotic gas and half a volume of oxygen gas condensed into one volume.

<div align="center">Azote.   Oxygen.</div>

Nitrous gas is formed of 1 vol. + 1 vol. constituting 2 vols.
Protoxide of azote 1   + $\frac{1}{2}$ 1

There seems no reason, therefore, to doubt that if nitrous gas could be deprived of half its oxygen, it would be converted into protoxide of azote, and that, by this abstraction, its bulk would be reduced exactly one half. But upon putting this apparently accurate conclusion to the test of experiment, the result turns out considerably different from what would be expected.

Every chemist is aware of the fact that nitrous gas is converted into protoxide of azote by keeping in it a quantity of moistened iron wire. The fact was first observed by Dr. Priestley, and led to the original discovery of protoxide of azote. The experiment must have been very often repeated since, though I am not aware that any very precise trials have been made to determine the change of bulk which takes place. It may be worth while, therefore, to relate here the result of a very careful experiment which I made to determine this point.

Fifteen cubic inches of nitrous gas (containing one per cent. of azotic gas) were let up into a glass jar standing over mercury, and previously filled with that liquid. The barometer stood at 29·08

inches, and the thermometer at 54°. Had the barometer stood at 30 inches, and the thermometer at 60°, the bulk of the gas would have been 14·72 cubic inches. Or (abstracting the azotic gas) 14·5728 cubic inches, I let up into this gas 50 grains of clean iron wire, together with a little water, just sufficient to moisten the iron. The iron began slowly to rust, and the nitrous gas to diminish in bulk. But as the experiment was made in winter, when the cold was severe, the diminution went on but slowly. After the interval of a month, it seemed to have reached its maximum. I allowed it to remain a fortnight longer, however, and then measured the residual gas. It amounted to nine cubic inches, the barometer standing at 29·94 inches, and the thermometer at 42°. This would have been 9·317 cubic inches if the barometer had stood at 30 inches, and the thermometer at 60°; or, abstracting the azote, 9·1698 cubic inches. This residual gas was pure protoxide of azote; for, being left standing upon water for 24 hours, it was totally absorbed by that liquid, except the 0·1472 cubic inch of azotic gas with which the nitrous gas had been originally contaminated.

Now this residual gas, instead of one half, amounts to very near two-thirds of the residual gas. I ask, how this is to be explained? It is true that in other experiments (made in warm weather) I have seen the bulk of the nitrous gas reduced almost exactly to one half of its original bulk; but the experiment above related was made with rigid attention to accuracy. Is it not probable from this that the specific gravity of the gaseous residue is not always the same? The subject obviously requires further elucidation.

The common opinion entertained respecting the way in which the oxygen is abstracted from the nitrous gas in this experiment is this. The iron decomposes the water in contact with it, uniting with its oxygen, and setting free the hydrogen. The nascent hydrogen acts upon the nitrous gas, abstracting half the oxygen, and thus converting it into protoxide of azote. Austin and Davy, and probably other chemists, have shown, that a little ammonia is evolved during this process. Now there is one thing that makes me hesitate about adopting this explanation, simple as it is, as the true one. After the bulk of the gas has decreased so as to reach its maximum, I have never observed any increase to take place; yet if the process had consisted in the decomposition of water by the iron, that decomposition ought to go on as long as any water continues in contact with the iron; and after the nitrous gas has been converted into protoxide of azote, the hydrogen, as it does not decompose the protoxide of azote, ought to mix with it, and increase its volume. But I have never found any hydrogen gas mixed with the protoxide of azote in this experiment; and what appears still more conclusive, if possible, I have placed iron wire in contact with a little water in a glass vessel over mercury, and allowed it to remain beside

another jar, in which the nitrous gas was converted into protoxide of azote by moist iron, yet the iron wire yielded no air bubbles during the whole time the process lasted.

II. *Phosphorous Acid.*

Berzelius has endeavoured to prove, in a paper which he published some years ago, that the oxygen in phosphorous acid is to that in phosphoric acid as the number 3 to 5. The composition of the two acids, according to his view of the subject, is as follows :

Phosphorous acid composed of. . . 2 phosphorus + 1·5 oxygen
Phosphoric acid. 2				+ 2·5

so that, in order to obtain whole numbers for oxygen, we must make the weight of an atom of phosphorus 4, and consider phosphorous acid as a compound of one atom phosphorus and three atoms oxygen, and phosphoric acid as a compound of one atom phosphorus and five atoms oxygen.

A set of experiments which I published some years ago seem to me to demonstrate the constitution of these two acids in a satisfactory manner. Phosphuretted hydrogen gas is of the specific gravity 0·9022. It is composed by weight of

Phosphorus . . . . 12 or 1·500
Hydrogen. 1				0·125

Therefore, if it be a compound of an atom of phosphorus and an atom of hydrogen, an atom of phosphorus must weigh 1·5. This gas for complete combustion requires either its own volume of oxygen gas, or 1¼ time its own volume. Now this gas consists of one volume of hydrogen gas combined with one volume of the vapour of phosphorus, and both condensed into one volume ; therefore the hydrogen in this gas will require half a volume of oxygen gas to convert it into water ; so that the phosphorus in a volume of phosphuretted hydrogen gas is capable of combining either with half a volume of oxygen gas, or with a whole volume. In the first place, phosphorous acid is formed, and in the second place phosphoric acid. By these experiments (which were made with the utmost care) it is demonstrated, I conceive, that phosphoric acid contains just twice as much oxygen as phosphorous acid combined with the same weight of phosphorus. These experiments are much more decisive than those of Berzelius, and they have the advantage of being much more easily made. All that is necessary is to procure phosphuretted hydrogen gas in a state of purity.

The specific gravity of phosphuretted hydrogen gas being 0·9022, and hydrogen gas not altering its bulk when it is converted into phosphuretted hydrogen gas, it is obvious that if we subtract the specific gravity of hydrogen gas from that of phos

phuretted hydrogen gas, the remainder will be the weight of phosphorus existing in a volume of phosphuretted hydrogen gas.

    Specific gravity of phosph. hydrogen 0·9022
    Specific gravity of hydrogen . . . . . . 0·0694
                                             ‾‾‾‾‾‾‾‾‾
    Phosphorus. . . . . = 0·8328

This number 0·8328 may be considered as the specific gravity of a volume of phosphorus in the state of vapour. We have then phosphorous acid composed of

    1 volume vapour of phosphorus .. = 0·8328
    ¼ volume of oxygen gas. . . . . = 0·5555

But 8328 : 5555 :: 1·5 : 1 very nearly. Hence it follows, that if an atom of phosphorus weighs 1·5, phosphorous acid is a compound of one atom phosphorus + one atom oxygen; and that the weignt of an integrant particle of it is 2·5.

Phosphoric acid is composed of

    1 volume vapour of phosphorus. . . = 0·8328
    1 volume oxygen gas. . . . . = 1·1111

But 8328 : 11111 :: 1·5 : 2 very nearly. Therefore phosphoric acid is a compound of one atom phosphorus = 1·5 and two atoms oxygen = 2; and the weight of an integrant particle of it is 3·5.

There exists but little difference of opinion between Berzelius and myself respecting the composition of phosphoric acid. The numbers resulting from my experiments are,

    Phosphorus. 100
    Oxygen 133¼

While the numbers of Berzelius are,

    Phosphorus . . . . 100·00
    Oxygen . . . . 128·17

Now my numbers are corroborated by the direct experiments of Sir Humphry Davy, who found that 100 phosphorus, when burned in a high temperature, absorbed 135 of oxygen. The mean of the experiments of Berzelius and Davy gives us the following numbers for the constituents of phosphoric acid:

    Phosphorus . . . . 100·00
    Oxygen. 131·58

These facts seem to me to lead us without any hesitation to admit phosphoric acid to be a compound of 1 atom phosphorus = 1·5 + 2 atoms oxygen = 2.

It is with respect to the composition of phosphorous acid that

the principal difference of opinion exists. I consider it as composed of

Phosphorus. 100
Oxygen 66⅔

Berzelius, as composed of

Phosphorus. .... 100·00
Oxygen .... 74·94

Berzelius ascertained the composition of phosphorous acid in the following manner: He dissolved 2·211 parts of protochloride of phosphorus in water. Now it is known that when this compound comes in contact with water, its two constituents are converted respectively into muriatic acid and phosphorous acid at the expense of a portion of the water, the chlorine uniting with the hydrogen of that liquid; while the phosphorus unites with its oxygen. He threw down the muriatic acid by means of nitrate of silver. The fused chloride of silver weighed 6·915. Now the chlorine in this portion of chloride of silver amounts to 1·705. This weight of chlorine to become muriatic acid must combine with 0·0474 hydrogen. This hydrogen it acquired by the decomposition of water, and this hydrogen must have been in combination with eight times its weight of oxygen, or 0·3792, which must be the quantity of oxygen that combined with the phosphorus in 2·211 parts of protochloride of phosphorus. But the phosphorus in the chloride was 2·211 − 1·705 = 0·506; so that, according to this experiment, phosphorous acid is composed of

Phosphorus .... 506·0 or 100·00
Oxygen. .... 379·2   74·94

But the consequences deduced from this experiment depend upon assumptions which have not yet been demonstrated. If the protochloride of phosphorus, have the property of absorbing more chlorine than an atom, which seems to be the case, if the analysis of it by Davy be nearly accurate, then the whole conclusions deduced by Berzelius from the experiment, as far as the constitution of phosphorous acid goes, fall to the ground. Now Davy found protochloride of phosphorus a compound of

Phosphorus. .... 2·98 or 1·500
Chlorine................. 10·62   5·345

Here the proportion of chlorine is almost one-fifth greater than an atom.

But let us consider protochloride of phosphorus as a compound of one atom chlorine and one atom phosphorus. Its constituents must then be:

Chlorine. .... 4·5
Phosphorus .... 1·5

When it comes in contact with an integrant particle of water, the hydrogen of the water combines with the chlorine, and the oxygen with the phosphorus, constituting muriatic acid and phosphorous acid. Now water is composed of

        Oxygen. .... 1·000
        Hydrogen......................... 0·125

When an integrant particle of protochloride of phosphorus comes in contact with an integrant particle of water, a double decomposition takes place, and there are formed

1. An integrant particle of muriatic acid composed of

        Chlorine. .... 4·500
        Hydrogen......................... 0·125

2. An integrant particle of phosphorous acid composed of

        Oxygen . ... 1·0
        Phosphorus....................... 1·5

Thus it is not possible that phosphorous acid can be a compound of any thing else than

        1 atom phosphorus. .... = 1·5 or 100
        1 atom oxygen .... = 1·0   66⅔

even from the very experiment upon which Berzelius has founded his opinion.

I do not expect that the preceding train of reasoning will be viewed as satisfactory by Berzelius. His notions respecting the atomic theory are so different from mine, as are likewise his opinions respecting the composition of muriatic acid, that we differ entirely in the original data from which we respectively set out; but I think they will be admitted to be perfectly satisfactory by every one who has imbibed accurate notions respecting the atomic theory, and who is acquainted with the present state of the science.

Professor Berzelius will, I hope, forgive me for expressing my regret at the unfortunate opinions which he has adopted respecting the laws to which chemical combinations are subjected. These opinions have been the result of an almost infinite number of experiments, conducted with the most minute attention to precision; and they have been deduced with exquisite address as the final results of these experiments. No person can have a greater admiration for the talents and industry of this illustrious chemist than I myself have; and no one is more fully sensible of the numerous obligations under which chemistry lies to his sagacity and unrivalled analytical skill; yet I have not the smallest doubt that the fundamental opinions by which all his conclusions are regulated are inconsistent with the true chemical laws of nature

III. *Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat.* 56° 23′ 30″. *Above the Level of the Sea* 129 *feet.*

1819.	Morning, 8 o'clock. Mean height of		Even., 10 o'clock. Mean height of		Mean temp. by Six's Ther.	Depth of Rain. In. 100	No. of days.	
	Barom.	Ther.	Barom.	Ther.			Rain or Snow.	Fair
Jan.	29·476	37·290	29·472	37·839	38·839	4·20	18	13
Feb.	29·418	36·071	29·442	36·571	37·785	2·45	14	14
March	29·720	41·226	29·737	41·161	42·580	1·10	9	22
April........	29·722	44·500	29·704	43·530	45·666	4·50	12	18
May	29·855	50·350	29·852	47·420	51·129	1·15	12	19
June	29·728	54·466	29·713	51·400	55·433	2·35	17	13
July	29·895	58·613	29·896	56·549	60·064	1·20	7	24
Aug.	29·852	60·419	29·886	59·387	63·129	1·40	6	25
Sept........	29·766	52·466	29·740	52·100	53·866	1·70	10	20
Oct.	29·728	44·806	29·754	44·419	46·677	4·20	14	17
Nov..........	29·640	35·300	29·670	35·700	36·700	1·80	11	19
Dec.	29·613	32·774	29·608	32·130	33·451	2·55	13	18
Aver. of year.	29·701	45·690	29·706	44·850	47·109	28·60	143	222

ANNUAL RESULTS.

MORNING.

BAROMETER.			THERMOMETER.		
Observations.	*Wind.*			*Wind.*	
Highest, Sept. 21 .... N W	30·45		July 24 .... W	....	67°
Lowest, Aug. 31 .... E	28·76		Dec. 11 .... N W	....	18°

EVENING.

Highest, Sept. 20 .... N W	30·40		Aug. 17 .... E	....	69°
Lowest, Jan. 17 .... N W	28·79		Dec. 10 .... N W	....	17°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	217	N and N E	28
Rain or Snow................	148	E and S E	109
	———	S and S W	60
	365	W and N W................	168
			———
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, December 11, Wind N W......................... 11°
Hottest, August 17, Wind E.............................. 80
Mean temperature for 1819............................... 46·109°

Result of three Rain Gauges.   *In.*

No. 1. On a conical detached hill above the level of the sea 600 feet.... 22·86
No. 2. Centre of the garden, 20 feet................................... 28·60
No. 3. Kinfauns Castle, 129 feet .... 30·20

Mean of the three gauges... 27·05

V. *An Account of the State of the Barometer, Thermometer, and Wind, during the late Hurricane, at the Island of St. Thomas's, in the West Indies, as observed on Board H.M.S. Salisbury.* Communicated by Col. Beaufoy, F.R.S.

1819.	Time.	Barom.	Ther.	Wind.	Remarks.
Sept. 20	8 P. M.	29·97		N by E	
21	8 A. M.	29·85		N	
	Noon.	29·77			
	1ʰ 35′	29·72	82°		
	1 56	29·72		NW by N	
	2 15	29·70			
	2 45	29·70			
	3 15	29·67			
	3 55	29·65			
	7 20	29·55			
	8 35	29·50			
	9 20	29·40			
	9 55	29·40		WNW	
	10 30	29·30			
	11 00	29·25			
	11 45	29·15			
22	3 30	28·80		SW	
	4 15	29·25			
	8 15	29·50			
	12 30	29·75			Height of the hurri-
	4 00	29·75	70		cane with thunder
	7 20	29·72			and lightning.

V. *Method of determining the Specific Gravity of Gases.*

In the first volume of the Memoirs of the Wernerian Natural History Society of Edinburgh, the members of that Society did me the honour to insert a paper of mine on olefiant and carburetted hydrogen gases. In that paper I have given an account of the method which I have long been in the habit of following for taking the specific gravity of gases. I have reason to believe that this method has been generally followed in Great Britain ever since I made it known. But if we are to judge from the account which Biot gives of the mode of taking the specific gravity of gases in his Traité de Physique, and from the description which Berzelius gives of his own experiments to determine the specific gravity of sulphurous acid gas,* it does not appear to me that the chemists on the continent are aware of the facility with which the specific gravity of gases may be taken.

My method is founded on a well-known fact that when two gases are mixed their bulk does not alter, I have a large glass flask fitted with a stop-cock. I weigh this flask as accurately as possible; then exhaust it, and weigh it again. The loss of weight sustained is equal to the quantity of common air drawn out, and is less or more according to the size of the flask and the goodness of the exhaustion. Let it be = a. I then fill the flask with the gas whose specific gravity I want, taking care to exhaust the

* See the last number of the *Annals of Philosophy.*

stop-cocks connected with the apparatus before the gas is let in All the precaution necessary is to take care that no particles of water or mercury (supposing the gas to be standing over mercury) insinuate themselves into the flask. It is obvious that the volume of gas which will enter the flask will be precisely equal to the volume of common air that has been previously drawn out of it by the air-pump. I now weigh the flask thus filled with the gas whose specific gravity I wish to know. The increase of weight of the flask above its weight when exhausted gives exactly the weight of the gas introduced into the flask. Let this weight be $= b$.

We have now obtained the weight b of a certain unknown volume of gas, and the weight a of exactly the same volume of common air. Now I say that the specific gravity of the gas is $= \frac{b}{a}$. Or we have only to divide the weight of the gas b by the weight of the air a. This quotient is the true specific gravity of the gas without any correction whatever being requisite either for temperature, or for the height of the barometer; because all gaseous bodies undergo the same change of volume by the same application of heat or pressure. Hence the specific gravity of air bears the same ratio to that of the gases at every temperature, and under every pressure.

Had Berzelius employed this method, he would not have required three days to obtain a very imperfect approximation to the specific gravity of sulphurous acid. Whoever will take the specific gravity of this gas in the way that I have directed above will find it just double the specific gravity of oxygen gas ; or 2·2222.

It very seldom happens that the gas whose specific gravity is taken is perfectly free from some admixture of common air. In such cases it is always necessary to determine the volume of air in the gas, and when this is known, the specific gravity of the pure gas may be deduced from that of the mixture by a very simple calculation. Let $x =$ specific gravity of the pure gas, $A =$ the volume of air in the mixture, $a =$ specific gravity of air; $B =$ volume of pure gas present, $c =$ specific gravity of the mixed gas, then

$$x = \frac{(A + B) c - A a}{B}.$$

VI. *Combinations of Prussic Acid.*

My facetious correspondent, who subscribes himself " Jack Addle," would have spared his animadversions on Count Le Maistre's paper, if he had recollected that mistaken inferences in science, when the mistake is obvious, and has been pointed out even before it has been committed, can never be in the least injurious to the progress of science ; while a haughty rejection of the first attempts at experiment, even though these attempts

should not be very successful, may damp the ardour of some person to whom hereafter the science may lie under obligations. Chemistry was nearly losing the unrivalled abilities and industry of Scheele by a piece of carelessness, for I will not call it haughtiness, of Bergman. Scheele, a young man whom nobody knew, sent a paper to Bergman, then in the height of his reputation, purporting to be a mode of obtaining tartaric acid in a state of purity from tartar. Bergman threw aside the paper without looking at it. Scheele of course was provoked. He withdrew his paper, and sent it to Retzius, who was a Professor at Lund. Had Retzius treated the paper in the same way that Bergman did, Scheele's name might never have been heard of. But fortunately for the science of chemistry, and for the reputation of Sweden, Retzius sent the paper to the Stockholm Academy, by whom it was published.

That Count Le Maistre's supposed new prussiates are mere mixtures of prussiate of iron with the different earths, &c. which he employs cannot escape the notice of any one who is acquainted with the action of acids on yellow prussiate of potash. One hundred grains of this salt, when treated with sulphuric acid, deposit 33 grains of pure prussiate of iron. Muriatic acid will probably occasion the same deposit. It is this portion of prussiate of iron thus evolved that occasions the blue colour in all the Count's experiments. But what then? Is it not possible that some of the mixtures which he points out may be useful as pigments? Or may not his views lead to the discovery of some useful pigment? These were my motives for inserting the paper in question in the *Annals of Philosophy*, and they seem to me to be perfectly legitimate. My readers would judge very erroneously if they were to conclude that I adopt or admit all the views given in the different papers published in the *Annals of Philosophy*. The authors alone are responsible for the opinions which they give. If a paper possesses ingenuity, plausibility, or novelty, and if it may have a tendency to excite others to useful researches, I never hesitate to insert it, though it should be contrary to all my own preconceived opinions.—T.

VII. *Geographical Position of Modena.*

In the year 1808 Baron von Zach made a set of observations to determine the latitude and longitude of the Lower Guirlandina, in Modena. He obtained the following results:

$$\text{Latitude N.} \dots\dots\dots\dots\dots\dots\dots\dots 44° \ 38' \ 55{\cdot}9''$$
$$\text{Longitude E. from Ferro} \dots\dots\dots 28 \quad 34 \quad 59{\cdot}2$$

He found the position of the Lower Asinelli as follows:

$$\text{Latitude} \dots\dots\dots\dots\dots\dots\dots\dots\dots 44° \ 29' \ 45{\cdot}1''$$
$$\text{Longitude} \dots\dots\dots\dots\dots\dots\dots\dots 29 \quad 0 \quad 9{\cdot}2$$

It is curious that the position of this city, one of the chief towns in Italy, the seat of a sovereign prince, of an Italian society, of several celebrated astronomers, and a well-known astronomical instrument maker, was never determined till a German astronomer took the trouble to determine the point as late as the year 1808.—(Correspondence Astronomique, i. p. 403.)

VIII. *Cadmium.*

Dr. Clarke, Professor of Mineralogy at Cambridge, has discovered cadmium in the radiated blende of Derbyshire. This discovery has been confirmed by several chemists in London, who have detected it likewise in other ores of zinc. A detailed account of Dr. Clarke's experiments will be given in a future number.

IX. *On fattening Pigs.* By Mr. J. Murray.

It is stated in an English paper that a pig belonging to Mr. Fisher, of Scrooby Inn, gained, by feeding on *Indian* corn, in the course of *six weeks and three days*, the enormous weight of *fifteen stone.*

The pigs in the vicinity of Naples are so fat as to be able to move only with difficulty; and I was curious to learn in what manner this desideratum was obtained. I was informed that the pigs were always fed in the first instance with *Indian corn,* and then generally permitted to shift for themselves. The method adopted by the Neapolitans to ascertain when the animal is ripe for the knife, is as extraordinary as it is cruel. An iron probe is plunged into the side of the animal, and when the point touches the muscular fibre, it is indicated by the expression of pain. The above fact is here corroborated, and the agriculturist may advantageously avail himself of the discovery.

X. *Varnish for Wood.* By Mr. J. Murray.

In vol. xi. pp. 119 and 371 of the *Annals of Philosophy*, Mr. Gill has afforded some excellent remarks on the French varnish for cabinet work, &c.

The cabinet work at Rome seemed to me truly beautiful. I was informed by the workmen that in order to produce the effect, olive oil was first used, and a solution of gum arabic in boiling alcohol afterwards applied to the surface.

ARTICLE X.

Magnetical and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37' 44·27" North. Longitude West in time 1' 20·93".

Magnetical Observations, 1820. — Variation West.

Month	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Jan. 1	8ʰ 45'	24° 35' 32"	1ʰ 35'	24° 37' 19"		
2	8 45	24 33 55	1 30	24 38 20		
3	8 50	24 33 57	1 30	24 36 17		
4	8 45	24 34 34	1 25	24 37 08		
5	8 45	24 33 36	1 15	24 38 04		
6	8 45	24 36 14	1 15	24 39 26		
7	— —	— — —	1 15	24 39 28		
8	8 45	24 41 30	1 15	24 40 37		
9	8 45	24 36 01	1 30	24 39 48		
10	8 45	24 34 32	1 30	24 38 08		
11	8 45	24 34 40	1 30	24 38 49		
12	8 45	24 34 33	1 35	24 38 39		
13	8 45	24 32 59	1 30	24 37 02		
14	8 45	24 33 06	1 15	24 35 56		
15	8 50	24 33 07	1 15	24 36 20		
16	8 50	24 32 30	1 25	24 43 21		
17	8 50	24 30 58	1 30	24 36 01		
18	8 50	24 34 10	1 20	24 38 14		
19	8 50	24 35 38	1 25	24 39 10		
20	8 50	24 34 07	1 15	24 37 42		
21	8 45	24 34 22	1 35	24 35 56		
22	8 50	24 35 59	1 25	24 39 34		
23	8 50	24 33 30	1 20	24 37 56		
24	8 50	24 34 41	— —	— — —		
25	— —	— — —	1 15	24 39 06		
26	8 50	24 32 45	1 20	24 37 13		
27	8 50	24 31 36	1 25	24 37 13		
28	8 50	24 34 21	1 30	24 36 13		
29	8 50	24 36 01	1 25	24 37 34		
30	— —	— — —	1 15	24 37 33		
31	8 50	24 33 16	1 30	24 38 18		
Mean for Month	8 48	24 34 06	1 26	24 37 54		

Evening column: Owing to the shortness of the days, evening observations discontinued.

In taking the mean, the morning observation of the 8th and the noon observation of the 16th are omitted, being unusually great, for which there was no apparent cause.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Jan.		Inches.				Feet.		
1	Morn....	28·948	22°	69°	NNE		Clear	21°
	Noon....	28·972	27	77	WNW		Foggy	27½
	Even....	—	—	—	—		—	22½
2	Morn....	29·121	24	71	S by W		Cloudy	
	Noon....	29·070	34	83	SSW		Rain	35
	Even....	—	—	—	—		—	29
3	Morn....	29·100	29	83	NW		Snow	
	Noon....	29·311	30	65	NW		Clear	30
	Even....	—	—	—	—		—	22
4	Morn....	29·600	24	82	WSW		Fine	
	Noon....	29·579	30	71	Var.		Fine	30
	Even....	—	—	—	—		—	20
5	Morn....	29·673	21	80	WSW		Very fine	
	Noon....	29·680	28	81	SSW		Very fine	31
	Even....	—	—	—	—		—	24
6	Morn....	29·635	30	83	WNW		Fine	
	Noon....	29·635	37	74¼	NNW		Fine	37
	Even....	—	—	—	—		—	30
7	Morn....	—	—	—	—		—	
	Noon....	29·828	31	64	E		Very fine	31¼
	Even....	—	—	—	—		—	22
8	Morn....	30·053	25	73	NE		Sn. show.	
	Noon....	30·053	27	65	ENE		Cloudy	27½
	Even....	—	—	—	—		—	19
9	Morn....	30·205	22	81	NE		Sn. show.	
	Noon.	30·205	28	63	NE		Fine	29¼
	Even....	—	—	—	—		—	19
10	Morn....	29·936	19	90	NNE		Fine	
	Noon....	29·900	25	68	NNE		Snow	26⅔
	Even....	—	—	—	—		—	23
11	Morn....	29·485	25	85	SSW		Sleet	
	Noon....	29·288	30	80	W by S		Fog, snow	32⅕
	Even....	—	—	—	—		—	25
12	Morn....	29·500	25	78	E		Cloudy	
	Noon....	29·600	23	72	ENE		Snow	24⅔
	Even....	—	—	—	—		—	16
13	Morn....	29·654	17	79	NNW		Fine	
	Noon....	29·630	26	70	N		Snow	26
	Even....	—	—	—	—		—	16¾
14	Morn....	29·719	22	71	ENE		Snow	
	Noon....	29·688	25	65	ENE		Snow	25
	Even....	—	—	—	—		—	11
15	Morn....	29·343	13	74	NNW		Clear	
	Noon....	29·243	19	68	W		Very fine	28
	Even....	—	—	—	—		—	18½
16	Morn....	29·200	22	91	NNW		Clear	
	Noon....	29·237	28	69	WNW		Clear	28⅔
	Even. ...	—	—	—	—		—	22
17	Morn....	29·174	28	90	SW		Cloudy	
	Noon....	29·100	33	85	SSW		Cloudy, fog	33½
	Even....	—	—	—	—		—	25
18	Morn....	28·905	27	84	ENE		Snow	
	Noon....	28·883	29	83	ENE		Snow	45
	Even....	—	—	—	—		—	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Jan.		Inches.				Feet.		
19	Morn....	28·367	45°	93°	WSW		Stormy	29°
	Noon....	28·500	44	76	W by S		Cloudy	45
	Even....	—	—	—	—		—	28
20	Morn....	29·100	28	76	NW		Very fine	28
	Noon....	29·090	33	60	E by S		Cloudy	34
	Even....	—	—	—	—		—	29
21	Morn....	28·660	33	68	SW by S		Snow	29
	Noon....	28·705	33	77	S by E		Snow	34
	Even....	—	—	—	—		—	20
22	Morn....	29·552	22	67	NE by N		Clear	20
	Noon....	29·625	29	66	ENE		Clear	30
	Even....	—	—	—	—		—	23
23	Morn....	29·609	28	71	SSW		Cloudy	23
	Noon....	29·587	35	73	SW		Fine	36
	Even....	—	—	—	—		—	31
24	Morn....	29·354	35	77	S		Cloudy	31
	Noon....	29·260	—	84	SSW		Rain	42
	Even....	—	—	—	—		—	36
25	Morn....	29·300	—	96	S by E		Fog	36
	Noon....	29·293	42	66	SSE		Rain	44½
	Even....	—	—	—	—		—	38
26	Morn....	29·019	43	94	W		Rain	38
	Noon....	29·156	45	85	WSW		Rain	48
	Even....	—	—	—	—		—	42
27	Morn....	29·144	45	91	SW by W		Very fine	42
	Noon....	29·100	49	77	SW by W		Fine	50
	Even....	—	—	—	—		—	41
28	Morn....	29·115	42	87	N		Cloudy	41
	Noon....	29·285	43	77	NNW		Cloudy	43
	Even....	—	—	—	—		—	36
29	Morn....	29·716	37	70	N by W		Cloudy	36
	Noon....	29·704	42	67	Var.		Fine	41⅔
	Even....	—	—	—	—		—	36¼
30	Morn....	29·600	—	97	SW		Fog, rain	36¼
	Noon....	29·589	43	92	WSW		Fog	44
	Even....	—	—	—	—		—	39
31	Morn....	29·544	40	87	SSW		Fine	39
	Noon....	29·539	44	79	SW		Cloudy	44⅚
	Even....							

Rain, by the pluviameter, between noon the 1st of January and noon the 1st of February, 1·02 inch. Evaporation, during the same period, 0·36 inch.

Article XI.

METEOROLOGICAL TABLE.

———

1820.	Wind.	BAROMETER. Max.	Min.	THERMOMETER. Max.	Min.	Evap.	Rain.	Hygr. at 9 a. m.
1st Mo.								
Jan. 1	N W	29·70	29·48	30	15			86
2	S W	29.70	29·60	39	29		—	75
3	N W	30·18	29·68	33	18			92
4	S W	30·30	30·15	26	14			92
5	N W	30·31	30·25	29	17			98
6	W	30·36	30·25	35	27			98
7	E	30·62	30·36	33	22		—	81
8	N E	30·81	30·62	30	20		—	72
9	N E	30·82	30·53	32	16		—	71
10	N E	30·53	30 11	29	21		—	76
11	S W	30·11	29·82	34	25		—	100
12	E	30·28	30·11	27	8		—	85
13	N W	30·35	30·27	29	17		—	85
14	E	30·35	30·02	24	0		—	73
15	N E	30·02	29·86	29	2			71
16	N W	29·86	29·80	32	18			87
17	S W	29·80	29·55	57	27			98
18	N E	29·55	29 95	47	28		1·15	99
19	S W	29·69	28 95	48	27			96
20	N W	29·70	29·23	37	24		—	77
21	Var.	30·16	29·22	38	20		—	77
22	N	30·25	30·16	32	13			70
23	S W	30·24	29·99	39	31			76
24	S W	29·99	29·94	45	38	41	38	79
25	S E	29 91	29·55	47	39		13	100
26	S W	29·70	29·55	50	44		07	100
27	S W	29·70	29·60	52	40		—	83
28	Var.	30·28	29·64	45	37			100
29	N W	30·28	30·22	45	37		—	94
30	S W	30·22	30·14	46	40			100
31	S W	30·14	30·09	48	30	30	10	82
		30·82	29·22	52	0	0·71	1·83	100—71

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

First Month.—1. Hoar frost: cloudy. 2. Much rim on the trees: rainbow: thaw: rainy. 3. Fair: *Cirrocumulus:* some snow this morning. 4. Hoar frost: foggy. 5. Very dense fog, mixed with the smoke of the city, a. m.: much rime on the trees, and hoar frost permanent. 6. Hoar frost and fog, a. m.: misty evening: slight thaw. 7. Clear and very cold: a little snow about 11, p. m. 8. Fine clear morning: barometer, 30·62 in. at nine, a. m.: some snow in the evening. 9. Barometer, 30·81 in. at nine, a. m.: some snow in the forenoon at temp. 26°. 11. Snow last night and this morning to about an inch depth. 12, 13. Snowy. At Tottenham the snow was observed to fall in very regular stellar crystals, and the feathered tribes appeared to suffer greatly from cold and hunger. 14. A brilliant *aurora borealis* between 11 and 12, p. m. from N W to N. 15. The temperature at sun-rise was, by the thermometer at Tottenham, about half a degree below *zero*. The thermometer at the laboratory, and another at Bromley, within a short distance, indicated *zero* as the minimum. The observation at 2° relates to the temperature at nine, a. m.: at 10, it was 3°; at 11, 5°; at 12, 7°; at half-past two, p. m. 18°; at six, 21°; at half-past seven, 25°. During this time the barometer fell two-tenths of an inch: the sky was overcast. 16. Very fine day. Temperature at nine, a. m. 19°; at 11, 26°; at two, p. m. 24°; at five, 21°; at nine, 25°. 17. Foggy morning: gentle thaw, followed by frost at night. 18. Snow from half-past four, a. m. to the depth of about two inches: thaw began about 10, a. m.: night rainy and boisterous. 19. Snow nearly gone this morning: the river full of floating ice of great thickness. 20. Froze again about four, a. m. The river much swollen, and an immense quantity of drift ice: about six, p. m. began a heavy fall of snow, carried by a wind from E and S E: about four inches in depth fell. 21. A gentle thaw: wind gone down: snowy, p. m. and sharp frost at night: the river still blocked up with ice: snow, p. m. followed by frost. 22. The barometer has risen 0·94 in. in the last 24 hours: very fine morning. 23. Hoar frost: lunar halo and corona at night. 25. Very stormy night. 26. A complete overflowing of the river: the marshes form one continuous sheet of water. 27. Fine morning: wet evening. 28—30. Cloudy. 31. *Cirrocumulus.*

RESULTS.

Winds: N, 1; NE, 5; E, 3; SE, 1; SW, 11; W, 1; NW, 7; Var. 2.

Barometer: Mean height

        For the month. .... 29·993 inches.
        For the lunar period, ending the 7th. .... 29·810
        For 14 days, ending the 6th (moon north) .... 29·740
        For 13 days, ending the 19th (moon south) .... 30·066

Thermometer: Mean height

        For the month...................................... 30·50°
        For the lunar period, ending as above .... 31·94
        For 30 days, the sun in Aquarius. .... 26·91

Hygrometer: Mean for the month 86

Evaporation.. 0·71 inch.

Rain .... 1·83

Laboratory, Stratford, Second Month, 21, 1820.　　　　　**L. HOWARD.**

ANNALS

OF

PHILOSOPHY.

APRIL, 1820.

ARTICLE I.

*Biographical Account of M. Werner, late Professor of Minera-
logy at Freiberg.**

ABRAHAM GOTLEB WERNER was born on Sept. 25, 1750,
at Wehran am Quiess, in Upper Lusatia.   Endowed by nature
with unusual quickness of understanding, and with the power of
extensive observation, he was also gifted with a happy faculty of
arrangement, a lively imagination, and a retentive memory.   In
conformity to the wish of his father, who had become the factor
of a Count Solmischen Eisenhammer, Werner devoted himself
from early youth to the same occupation.   He received the
rudiments of his education at the school of the Orphan Hospital
at Buntzlau in Silesia, and was afterwards placed at the Academy
of Freiberg ; and from the last mentioned place he went to study
at Leipzig.   Here, and during his whole life, Werner struggled
to acquire scientific information ; and while he gained for him-
self reputation for his proficiency in general literature and the
languages, he continued severe in judging of himself, and lenient
and indulgent towards others, mild, affectionate, and generous ;
he was a true patriot, and a citizen of the world in the most
honourable sense of the word.

It was at Leipzig, in the year 1774, that Werner, already more
distinguished for his study of natural history than for that of the
law, laid the firm foundation of those opinions relative to orycto-

gnosy, of which he was the contriver. He supplied, instead of
the confused mass of which this species of knowledge had
hitherto consisted, those compendious descriptions, delivered in
happily chosen, expressive, scientific language, which have
accomplished the difficult end of placing in an intelligible point
of view the principles of this science. The new method, from
the comprehensive nature of its illustrations, soon became exten-
sively known and adopted; and in 1780, Werner, in the transla-
tion of the system of Cronstedt, which he then published,
explained, in a connected shape, his method of classification, and
his opinions in general, illustrated, and improved, since their
first origin, by many alterations and additions. He published in
1791 a second account of his doctrines, after having received
considerable additions to his mineralogical knowledge from his
being employed in drawing a catalogue of the collection of mine-
rals formed by Mr. Pabst, of Ohain.

In the year 1775, not long after he had commenced his career
as an author, Werner obtained a permanent situation in the
Academy of Freiberg, the earliest cradle of mineralogical science
in Germany; and destined to flourish with renewed life in conse-
quence of his labours. He was appointed, in addition to a pro-
fessorship, superintendent of the museum, and here his active
temper for investigation and observation obtained a wide field,
and by his unrestrained and enthusiastic exertions, in spite of
much opposition, he raised in his favour a strong party feeling,
and general admiration. The attempts to persecute Werner,* ·
and to impede the introduction of his doctrines, had quite the
contrary effect to what their authors intended, and contributed
essentially to hasten the result so favourable and so brilliant to
him. The boundaries of the science were soon enlarged by the
effects of his favourite labours; geognosy, reduced to an intelli-
gible shape, a work entirely the creation of Werner, being consi-
dered henceforth as a part of the science. His theory of the
periods in the formation of mountains, his researches respecting
rocks, and the nature of their aggregation into the masses of
which the crust of the earth is composed, his reflections upon
the internal structure of mountains, his theory respecting veins,
his doctrine of the formations, and of the origin of the later

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* Among these may be classed the labours of Veltheim, Heinitzen, and others;
viz. Veltheim's essay, with remarks on the old and new nomenclature of minerals,
Helmstadt, 1793, a work deficient in argument; next comes the attack which Mr.
Chenevix hazarded against his preceptor, and which does not possess much merit,
published in the Annales de Chimie, 1808, tom. 65, p. 11, 113 et 225. The
reply to this is contained in a letter from D'Aubuisson to Berthollet, Annales
de Chimie, 1809, tom. 69, p. 155 et 228; also in Thomson's observations in answer
to M. Chenevix's attack upon Werner's mineralogical method, *Annals of Philoso-
phy,* vol. i. p. 245. Still less conclusive are the objections of the deceased Estner,
entitled " Unbiassed Thoughts respecting Werner's Improvements in Mineralogy."
Vienna, 1790. Compare these with what Karsten has said on the opposite side,
entitled " Upon Werner's Improvements in Mineralogy, occasioned by the Abbé
Estner's unbiassed Opinions." Berlin, 1793.

traps and volcanoes, will convey the name of Werner to the latest posterity. Geognosy, as he formed it, may be considered the philosophy of mineralogy, the fairest, and most perfect half of the philosophy of unorganised nature. Every question which is started on this subject, all objects connected with the structure of the earth, and which relate to the masses of which it is composed, are an appropriate exercise for an enlightened mind. Leibnitz, Descartes, Bacon, Burnet, Laplace, and all illustrious men of ancient and modern times, have respected this laborious species of research.

Werner laboured more by his lectures than by his writings, for he considered that the numberless works on mineralogical science, which he had consulted, had misled rather than instructed him, their authors appearing as if certain that the utmost extent of what was known on this subject was too imperfect for his attention .Science, however, has cause to rejoice that among the finished papers of Werner, which he bequeathed at his death to the Academy of Freiberg, many well-arranged manuscripts have been found, the publication of which fine legacy remains anxiously to be hoped for.

While the science which Werner had imposed upon himself as a law continued on his part, his doctrines, so far as they were known, were pirated by others; and (unchecked by the circumstance of Werner continuing by frequent changes and improvements to separate still further his opinions from theirs,) we have seen ourselves inundated with works relative to his theory, the authors of which did not follow the ideas of their preceptor, however numerous and enlarged they might be, but permitted themselves to indulge in speculations of their own with the most unrestrained freedom ; so that along with what is useful of Werner's, we possess much of what is foreign to him ; and as none of these authors have followed Werner's doctrines in their entire and original purity, none of them possess great value, nor bear the absolute marks of his authority ; while, on the contrary, he has opposed the opinions contained in many of them by strong and decided arguments.

In England and Italy, where, previous to the time of Werner, mineralogical researches had been less ardently prosecuted than in Germany, the new doctrines very soon found advocates. Kirwan adopted his method, as well as many other proselytes from the established system. Hawkins, Mitchell, and Weaver, formed part of the new school, and the latter published a meritorious translation of his work upon the external characters of minerals ; and lastly, Prof. Jameson, of Edinburgh, received his education at Frieberg.

On the side of Italy, Napione extended the doctrines of his master ; and in Denmark, the labours of Wad and Esmark procured him approbation and followers.

Brochant came from France to receive instructions from

Werner, and returned loaded with knowledge to his country; and while he attained the praise of founding a new school, had nearly received the punishment of exile from his native land.

After Brochant, other advocates of the school of Werner arose in France; but their services to the cause will not here detain us, with the exception of d'Aubuisson, who was the first who communicated to the public a just account of several of Werner's doctrines.

In order to be as concise as possible relative to the progress of the Wernerian doctrines in other foreign countries, I shall only relate that in Spain and America they made progress in consequence of the assertions of Herrgen and Del Rio; and that in Portugal, the disciples of this school were headed by Andrada; and the system extensively published and adopted.

Hitherto in speaking of Werner, we have only noticed his labours in geognosy and oryctognosy, the sciences in which he was destined to render himself immortal, and have spoken of his opinions on these subjects, through which, and his researches relative to the structure of the globe, he so anxiously endeavoured to direct the attention of his followers to the different branches of the science of mineralogy.

The most remarkable incident, however, in the later years of Werner was his journey to Paris in 1802, occasioned by his zeal in the cause of science, and the wish to confer with the naturalists of the French capital most devoted to his cause. This modest and fine feeling, learned man, although not insensible to the value of external honours, found himself on this occasion overwhelmed with multiplied proofs of the most flattering distinction, inspired by the disinterested knowledge of his worth.*

The cabinet which Werner left behind him,† (the result of a life spent in the laudable pursuits attending the formation of this collection, and the sacrifices which had attended its formation, afford convincing proofs of his earnest exertions in the cause of science), has a double value, derived in the first instance from the great merit of the individual who made the collection, and in the second, from the scientific knowledge displayed in the arrangement of the whole. This valuable collection is now in

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* Werner was freely admitted every where at Paris, and courted wherever his mineralogical knowledge could be appreciated. It is related that in the laboratory of the School of the Mines, when Descotils was occupied in an elaborate analysis of some specimens of iron ore, he excited astonishment by determining their standard and chemical constitution by a cursory examination of their external characters, in surprising correspondence with the latest analyses of them by the chemist.

† The collection is divided into six parts; viz. precious stones, oryctognosy, a collection of show, one of petrefactions, and one illustrative of the external characters of minerals. The collection of precious stones is one of surprising value and rarity. We have had only a very imperfect account of these collections, but it is to be hoped that we shall soon be put in possession of an ample description of them by some experienced individual.

the possession of the Academy of Freiberg, to whom Werner left it in the most disinterested manner.*

Werner belonged to most of the learned societies both of his own and of foreign countries. Our Royal Academy of Sciences possessed him as a member since the year 1808. A society founded in Edinburgh assumed his name as an honourable distinction,† and not long before his death he was constituted president of a society founded in his native country for the encouragement of that science, which lay under such obligations to him.‡

Thus lived Werner, and thus he laboured : his sacrifices on account of science made him renounce the happiness of becoming a husband, and a father, although from his amiable disposition, his cheerful and serene temper, he seemed particularly formed for the pure enjoyments of domestic life. Surrounded by a numerous circle of his friends and scholars, previous to his approaching dissolution, he freely communicated the whole of his knowledge ; and intimately and confidentially laid open his whole mind. Steadily true to the fulfilment of his duties, he was seen at the extremity of old age possessed of continued youthful vigour, full of the clearest views, and the brightest conceptions.

The estimable King of Saxony, the friend and patron of merit in whatever situation it may be found, distinguished him as a rare example of worth.§

Posterity will form a just and true conception of his high worth, and mankind will experience a great loss in his death. Werner did not exclusively belong to Saxony ; he was the benefactor of the world at large.

List of Werner's Writings.

Werner published at Leipzig in 1774 An Essay on the Exter-

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* An offer of 50,000 dollars was made from England for 100,000 of these specimens, but the patriotic proprietor left them for 40,000 dollars to the Academy of Freiberg. Of this sum he sunk 30,000 dollars in an annuity for himself, and an only sister; neither of them had any family; and the remainder of the money received from the Academy for his minerals, he left to it at the death of himself and his sister. He also left his exquisite collection of books and medals to the Academy for 5000 dollars. This contained 6000 Greek and Roman medals.

† The Wernerian Natural History Society. The seal of this Society has engraved upon it a likeness of Werner.

‡ The Mineralogical Society established at Dresden in the course of the winter of 1816 and 1817. The King of Saxony has in every way given encouragement and protection to this Society, and has granted it a particular seal and diploma.

§ Werner received a particular proof of this distinction in being decorated with the Grand Cross of the Royal Saxon Order of Merit. His birth has of late also been celebrated in public ; and we are allowed to hope that through the exertions of the Prussian Chevalier Gerard, we shall possess, in a well executed bust of Werner, by Posch of Berlin, a monument of him in a durable shape. In order to form a calculation of what may be the price of this bust, which will be sold for prime cost, the number of those who wish to be possessed of a cast is anxiously waited for, and it is hoped that the admirers of Werner will consider this invitation as opportune. The bust will be cast at the foundery of Gleiwig, in Silesia.

nal Characters of Minerals. This work was translated into
French, and published at Paris in 1790, by the translator of the
Memoires de Chimie de Scheele (Mlbe. Picardet).

In 1780, he published at Leipzig a Translation of Cronstedt's
Essay on Mineralogy from the Swedish, with Notes, and an
Account of the External Characters of Minerals.

In 1791 and 1792, he published A Full and Systematic Cata-
logue of the Cabinet of Mr. R. E. Pabst, of Ohain, which he
drew up, and edited in two volumes. This is, to appearance,
the description of the cabinet of a private individual, but its
contents prove that Werner took this opportunity to describe
how a collection ought to be arranged and described. Pabst,
of Ohain, was an amateur naturalist, who, from holding an offi-
cial situation under the Saxon government, possessed opportuni-
ties of collecting the rarest and most curious mineral specimens.
After his death, his heirs, in 1786, wished to give this collection
a permanent value, and proposed that Werner should undertake
its arrangement and description. In completing this work,
Werner followed the arrangement in use in describing a cabinet
of natural history; and as he spared no pains in giving an elabo-
rate account of the collection, he composed a work calculated to
be of the utmost service to science. The Journal des Mines,
vol. ii. chap. 91, p. 73, gives a copious summary of this work.

At Dresden, 1787, he published A short Classification and
Description of Mountains.

At Freiberg, in 1791, he published The New Theory of the
Formation of Veins, with Remarks on the Formation of Moun-
tains, particularly those in the Neighbourhood of Freiberg.
Translated into French, with notes, by D'Aubuisson.   Paris,
1802.

In the Miner's Journal, he published An Essay on the Basaltic
Formations of Scheibenberger Hill, together with the controver-
sial Correspondence with Mr. Voight on this Subject. A History
of the Characters and a Chemical Investigation of Apatite.
Vorkommendes Basaltes auf Kupper, vorzugtich hoher berge.
Remarks on Evermain's Description of a Basaltic Mountain,
called King Arthur's Seat, near Edinburgh; and its close Resem-
blance to Scheibenberger Hill.   Notes upon a Letter of Widen-
man's, relative to some Hungarian Fossils.   Remarks on a Letter of
the Chevalier Napione relative to the Tuberg Iron Mountain.
Description of the External Character of Prehnite, with some
Observations upon the Name which it has received, and addi-
tional Remarks upon the System of naming Objects in Natural
History from Individuals.   Description of the External Charac-
ters of Kyanite.   Description of the External Characters of
Olivin, Chrysolite, Beryl, and Chrysoberyl, with some additional
Remarks on the Gradations of the First.   Remarks on the Traps
of Sweden, with some Observations on the Origin and Applica-
tion of this Term, and what may probably be its future Fate,

and a short Attempt to determine the precise Species of Mountains to which it may in future be applied.

In Hoepfner's Helvetic Magazine of Natural History, he published An Attempt to explain the Origin of Volcanoes from the Inflammation of large Beds of Coal, and the Connexion of this Circumstance with the Formation of Basalt.

In Von Crell's Chemical Annals, On the Buzzen-Wacken of Joachimstal. 1789. Band 1, 8, 131.

In the Magazine of Medicine and Natural History, A Description of a new Ore of Silver (Arsenical Silver). A Letter from Leslie relative to a singular Specimen of Crystallized Gypsum found in an old Fortification.

ARTICLE II.

Observations on the Barometer, Thermometer, and Rain, at Manchester, from 1794 to 1818 inclusive.[*]   By John Dalton.

I. *Of the Barometer.*

IN a long series of observations it is scarcely to be expected that the same instruments can be used throughout. Accidents are occurring which either derange or destroy them. It is expedient, therefore, to notice such occurrences; as it seldom happens that instruments, particularly barometers, are replaced or renovated in like circumstances as before; and if this is not the case, they must necessarily mislead in comparison.

During the first period of five years, I had a barometer consisting of a straight tube of the usual length, and between one and two-tenths of an inch internal diameter; it was carefully filled with dry mercury, and inverted into a cylindrical cup containing mercury; the diameter of the cup was such as to require scarcely any sensible allowance for the rise and fall of the mercury in the tube. It suffered no material change, as may be inferred indeed from the annual means, till near the end of the period. During my absence in August, 1798, it had been in unskilful hands; a part of the mercury had been lost out of the cup, and probably a few atoms of air had got into the tube. Not being at that time particularly interested in meteorology, I contented myself with noting the daily observations as usual, without summing up the monthly means, or making any comparative observations. The consequence was, that some years elapsed before I was struck with the depression that had taken place in the mercury, which, upon examination, appears to have

[*] Read before the Literary Society at Manchester, Nov. 13 and Dec. 11, 1818.

been about two-tenths of an inch. It was not till after another period of five years that I determined to renew the barometer. A tube was taken of about one-seventh of an inch internal diameter, having a large bulb at the lower extremity; this was filled with mercury that had a few minutes before been boiled in order to expel the air and moisture more effectually. This was found to stand nearly two-tenths of an inch higher than the previous one, and a few hundredths higher than it stood at originally; owing in part at least to the elevation or height of the barometer, above the level of the sea, being 10 or 12 feet more in the first period than in the latter. In order to allow for the rise and fall of the mercury in the bulb, the scale of four inches was made only 3·98 inches, and subdivided into tenths as usual. The observations throughout the whole series were taken three times each day; namely, at 8 a. m. and at 1 and 11 p. m. The new or last mentioned barometer has been used for the last 15 years, commencing with January, 1804 ; and it may be right to observe, that for the three last months of 1798 it was used, but' the reduction was applied to the monthly means, in order to make the observations of the whole year uniform. As the adhesion of the mercury to the tube is more or less observable in great variations, I make it a practice to give one or two gentle vibrations to the mercury prior to any observation, which I think is more accurate than using a wider tube without such vibrations.

The elevation of my barometer above the level of the sea is nearly 180 feet; it is about 100 feet above the level of the Duke of Bridgewater's canal, and that is nearly 80 feet above the sea. This elevation is rather greater than would be deduced from the observations of the barometer for the last period of 15 years, in which the annual average height of the instrument is about 29·91. On the supposition that the average height of the barometer on a level with the sea is 30 inches, my barometer would be inferred to be about 90 feet above the sea; but I conceive the real average height of the barometer on a level with the sea has never been accurately determined. Possibly there may be some difference in the specific gravity of the mercury used for this instrument.

During so long a period of years, there must have occurred interruptions to the observations, but I never delegated any person to supply for me ; these interruptions, however, scarcely happened, except in the month of July, when they have been very frequent for half of the month. These blanks, however, have been filled up partly from the observations of Mr. Thomas Hanson, of this town, who has kept a meteorological journal with great attention for several years, and partly from the observations of the Royal Society and other authentic sources, due allowance being always made for the difference in the absolute heights of the two barometers.

Mean Height of the Barometer at Manchester.

Yrs.	Jan	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Mean
1794.	29·94	29·71	29·86	29·80	29·91	29·97	29·92	29·84	29·78	29·70	29·64	29·88	29·83
1795.	30·03	29·60	29·76	29·71	30·11	29·80	29·89	29·89	29·99	29·52	29·70	29·82	29·85
1796.	29·47	29·77	30·04	30·02	29·70	29·89	29·66	30·09	29·90	29·88	29·83	29·92	29·85
1797.	30·00	30·23	29·94	29·77	29·78	29·83	29·96	30·06	29·64	29·81	29·87	29·60	29·87
1798.	29·85	30·02	29·91	29·87	29·96	30·01	29·72	30·08	29·75	29·63	29·48	29·87	29·85
1799.	29·74	29·49	29·66	29·46	29·62	29·80	29·57	29·50	29·53	29·52	29·61	29·80	29·61
1800.	29·26	29·70	29·69	29·41	29·61	29·78	29·93	29·81	29·54	29·65	29·44	29·46	29·61
1801.	29·59	29·56	29·61	29·86	29·65	29·88	29·65	29·88	29·73	29·62	29·55	29·29	29·66
1802.	29·73	29·57	29·86	29·83	29·90	29·68	29·62	29·82	29·85	29·58	29·54	29·55	29·71
1803.	29·55	29·67	29·88	29·67	29·77	29·83	29·93	29·86	29·87	29·87	29·36	29·46	29·72
1804.	29·66	30·09	29·62	29·69	29·81	30·11	29·86	29·90	30·16	29·64	29·87	29·83	29·85
1805.	29·57	29·77	29·91	29·87	29·95	30·04	29·97	29·97	29·97	29·94	30·28	29·80	29·92
1806.	29·52	29·85	29·82	30·17	30·03	30·15	29·83	29·87	30·07	29·95	29·75	29·55	29·85
1807.	30·10	29·81	30·08	29·76	29·85	30·07	29·96	29·94	29·84	29·90	29·55	29·85	29·90
1808.	29·84	30·18	30·25	29·93	29·95	30·05	29·98	29·93	29·91	29·76	29·78	29·87	29·95
1809.	29·57	29·72	30·12	29·86	29·96	29·98	29·98	29·80	29·80	30·23	30·03	29·82	29·91
1810.	30·15	29·85	29·74	29·90	30·00	30·20	29·86	29·96	30·07	30·00	29·55	29·77	29·92
1811.	29·95	29·53	30·18	29·86	29·90	30·05	30·18	30·02	30·07	29·70	30·01	29·79	29·94
1812.	29·95	29·68	29·88	30·05	29·98	30·05	30·07	30·13	30·16	29·53	29·94	30·07	29·87
1813.	30·14	29·77	30·23	30·05	29·84	30·13	30·00	30·17	30·11	29·75	29·81	29·90	29·99
1814.	29·66	30·12	29·86	29·91	30·11	30·14	29·94	30·03	30·22	29·86	29·76	29·70	29·94
1815.	29·94	29·83	29·77	30·02	30·00	29·97	30·20	30·00	30·05	29·92	30·04	29·83	29·96
1816.	29·70	29·91	29·82	29·82	29·93	30·02	29·74	30·05	29·99	29·94	29·86	29·80	29·88
1817.	29·84	29·97	29·68	30·35	29·87	29·26	29·80	29·83	30·09	30·11	30·02	29·66	29·85
1818.	29·82	29·79	29·54	29·76	30·01	30·12	30·15	30·17	29·85	29·95	29·91	30·14	29·93
Mean	29·78	29·81	29·87	29·86	29·89	29·98	29·89	29·94	29·92	29·80	29·76	29·76	29·85
corrected for expan.	29·82	29·84	29·89	29·86	29·88	29·95	29·85	29·90	29·89	29·80	29·78	29·79	

In the above table, the means for each year are placed on the right, and the means for each month of the year at the bottom; by which we are enabled to judge whether any particular season or month of the year is more liable than another, to have an accumulation or deficiency of the atmosphere. It is evidently only from a long series of years that we are entitled to draw any conclusions of this kind.

From an inspection of the above table, it is obvious that the barometer appears to be higher in the summer than in the winter months; but this must arise in part at least from the expansion of mercury by heat; a correction, therefore, is necessary on that account; and the following table of temperature for each month in the year, with the known expansion of mercury, enables us to apply the proper correction. The correction being applied accordingly, we have the subjoined corrected heights for each month. On looking over this corrected column, we still perceive the mercury higher in the summer months than in the winter. The heights for March, April, May, June, July, August, and September, are all at or above the mean; and June in particular possesses a marked superiority of one-tenth of an inch above the mean. The heights of January, February, Octo-

ber, November, and December, are all below the mean; those for November and December are nearly one-tenth of an inch below the mean.

These results were quite new to me. On looking over the observations in the fourth volume of the Memoirs, however, I found that similar results had been obtained by Mr. Hutchinson, from an average of 25 years' observations at Liverpool; namely, from 1768 to 1792 inclusive; and this period being entirely anterior to the one above, is of course totally independent of any fortuitous events in that period. Mr. Hutchinson's results, corrected for temperature, will stand as under; namely,

Jan. 29·75; Feb. 29·61 : March, 29·82; April, 29·79; May, 29·78; June, 29·79; July, 29·78; Aug. 29·77; Sept. 29·67; Oct. 29·71; Nov. 29·67; Dec. 29·68; Mean, 29·73.

Here the months March, April, May, June, July, and August, are all above the mean, and the remaining six months are all below it, except January.

Wishing to have further corroboration of the fact, I had recourse to the register kept by order of the President and Council of the Royal Society, and collected the whole series of observations of the barometer for 38 years. I found the results, corrected for temperature, as under; namely,

Jan. 29·87; Feb. 29·86; March, 29·93; April, 29·85; May, 29·88; June, 29·93; July, 29·87; Aug. 29·91; Sept. 29·88; Oct. 29·84; Nov. 29·82; Dec. 29·84; Mean, 29·87.

Here again the months March, May, June, July, August, and September, are all at or above the mean; and January, February, April, October, November, and December, are all at or below the mean.*

By comparing all these together, it would seem to be established as a fact, that from March to September, the weight of the atmosphere is greater than from September to March in this part of the world. This cannot be ascribed to the account of rain; for the summer period contains wet months as well as dry; and in the London averages, the month of April is in the low period, though the driest in the year. It cannot be ascribed to temperature; for the month of November is warmer than March; yet this last is in the high period by all the tables, and the former in the low period. Nothing appears to me indicative of the periods but the declination of the sun; it seems that when

* I cannot refrain from the remark that some of the annual tables of the Royal Society's meteorology, exhibit marks of extreme carelessness. In the table for 1815, I found *four* out of the twelve monthly means of the barometer greatly erroneous: namely, January, April, May, and October. The monthly means for August, 1807, and September, 1808, are also *greatly* wrong. These errors were detected by a comparison with my results for the same months; as I found the results at both places irreconcileable according to the known laws of barometry. As the above work, sanctioned by such authority, will naturally be regarded as a national standard in meteorology, it is greatly to be desired that the whole of the tables were revised and corrected.

the sun is north of the equator, the weight of the atmosphere increases in these parts, and again diminishes when south of the equator.

The means by which the effect is produced, I conceive, are these: the sun's action is constantly increasing the mass of aqueous vapour in the atmosphere during the period from the vernal to the autumnal equinox; that is, the whole mass of vapour existing in the atmosphere is daily increasing, notwithstanding the quantity precipitated. This fact is verified by the constant rise of the vapour point till the month of September, after which it commonly declines pretty rapidly. Now it is obvious that the addition of steam or aqueous vapour to the atmosphere must add to the weight of the atmosphere; and this is, I imagine, the cause of the increase of its weight in that season.

I am aware that another conclusion, the very opposite to this, may be deduced from the premises. Aqueous vapour, it may be said, is specifically lighter than dry air; and, on that account, the greater the quantity of aqueous vapour, the less is the weight in any given volume of air of given elasticity. But it must be remembered that the aqueous vapour at the most constitutes but $\frac{1}{50}$th part of the atmosphere, and any excess of this which may prevail in any one place, cannot be supposed powerful enough to move the rest of the atmosphere towards any other place, where the vapour is deficient. Now we have no reason to believe that much intercourse takes place between the atmospheres of the northern and southern hemispheres. The great and unceasing currents of air are between the equator and the polar regions; but that any large volumes of air cross the equator from one hemisphere to the other, does not appear from any phenomena we are acquainted with. And if the air does not cross the equator, the vapour cannot, being so intimately blended with the air. Thus, although there may be a constant pressure or tendency of the atmosphere in the northern hemisphere to invade that of the southern during our summer, and *vice versâ* in winter; yet I conceive it never can be so effectual as to restore a perfect equilibrium during the season, but will leave an excess of aqueous vapour in our hemisphere, unbalanced either by air or vapour of the opposite hemisphere.

II. *Of the Thermometer.*

My thermometer is situated out of a window on the second floor, about 16 feet above the ground, and about six inches from the wall; it has an eastern aspect, and open, airy situation, is not affected by the sun, except in a summer's morning, and it is then duly shaded to prevent the sun's influence. The observations are taken three times a day, as with the barometer, at 8 in the morning, and at 1 and 11 in the afternoon. I have some reason to think the observations give a mean temperature rather below than above the true mean. The temperature of

springs in this place is usually between 48° and 50°; probably, the mean annual temperature may be nearly 49°. The general annual mean, as determined by my thermometer, is between 47° and 48°. The monthly means for July I have had usually to borrow from Mr. Hanson's observations, or from those of others at some distance, as for the barometer.

Mean Height of the Thermometer at Manchester.

Yrs.	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Mean
1794.	31·7°	42·3°	42·4	49·7°	49·5°	59·9°	62·0°	59·5°	54·1°	47·7°	41·0°	36·2°	48·0°
1795.	24·3	31·4	37·8	44·9	46·0	55·0	61·0	61·0	60·9	51·9	38·7	43·0	46·4
1796.	44·0	37·6	41·4	52·7	52·0	56·9	57·5	61·0	60·3	47·9	42·0	31·3	48·7
1797.	40·6	41·3	41·4	47·2	54·0	57·7	61·0	60·0	55·7	48·4	42·5	41·0	49·2
1798.	40·4	39·4	42·0	52·9	57·7	64·5	61·0	61·0	53·0	46·7	39·8	34·4	49·4
1799.	34·6	36·2	37·3	47·0	47·0	55·2	57·0	55·0	53·5	45·2	40·4	33·5	44·6
1800.	36·5	35·6	38·8	47·3	51·8	53·1	58·8	60·7	55·4	47·1	40·1	37·3	46·9
1801.	39·3	39·4	42·0	46·5	51·9	56·3	58·0	62·1	56·0	49·2	39·6	34·6	47·9
1802.	34·1	38·1	41·0	47·2	52·1	55·2	55·3	61·3	57·0	49·1	43·5	38·9	47·4
1803.	35·1	38·2	42·7	47·4	49·5	55·6	61·7	58·7	51·7	47·9	39·9	37·8	47·3
1804.	43·0	37·1	40·3	43·0	54·4	58·6	57·0	57·4	57·3	50·7	43·6	36·0	48·2
1805.	34·3	38·9	43·6	46·1	51·2	56·0	61·0	62·0	57·8	46·1	40·7	37·5	48·0
1806.	38·1	38·8	40·0	43·7	52·2	58·1	59·0	58·9	54·5	49·6	43·8	41·9	47·5
1807.	36·3	38·1	36·8	46·5	53·4	56·1	61·2	62·1	50·1	51·7	37·0	35·0	47·0
1808.	36·2	37·0	37·6	42·5	57·2	57·7	64·0	61·1	55·0	44·3	42·5	36·2	47·6
1809.	34·0	42·0	42·5	41·7	55·2	55·2	57·5	57·5	53·3	50·1	40·0	38·7	47·3
1810.	35·0	37·5	40·5	47·1	48·2	57·9	58·8	58·8	56·8	48·6	41·5	38·5	47·4
1811.	34·2	39·6	45·4	48·6	55·0	57·8	59·5	58·7	57·1	53·4	46·1	36·4	49·3
1812.	36·9	41·3	38·2	42·6	52·7	56·2	56·0	58·8	57·1	49·0	40·9	35·5	47·1
1813.	35·1	42·4	44·7	47·2	54·6	58·5	60·0	58·9	55·5	47·4	39·1	36·7	48·3
1814.	26·5	34·5	37·3	50·1	49·5	55·1	58·0	58·6	55·1	45·8	38·0	35·9	45·4
1815.	29·2	39·3	40·9	45·6	51·0	54·7	54·0	56·9	54·2	51·0	40·5	36·5	46·2
1816.	37·2	36·7	40·0	45·6	51·1	56·6	57·0	58·3	55·2	51·7	39·6	37·5	47·2
1817.	39·8	43·6	42·6	47·2	49·2	59·5	57·5	57·4	57·7	45·3	48·5	34·0	48·5
1818.	38·8	36·6	38·0	44·0	53·6	61·4	66·0	59·8	56·8	54·0	49·0	38·7	49·7
Mean	35·8	38·5	40·6	46·3	52·0	57·2	59·4	59·4	55·6	48·8	41·5	36·9	47·7
Max.	44·0	43·6	45·4	52·9	57·7	64·5	66·0	62·1	60·9	54·0	49·0	43·0	
Min.	24·3	31·4	36·8	40·4	46·0	53·1	54·0	55·0	50·1	44·3	37·0	31·3	

III. *Of Rain.*

In the 5th volume of the Memoirs, Part II, published in 1802, I have given an account of the depth of rain which fell in Manchester during the eight preceding years, with the average monthly and annual means. Having now a further series of observations for 17 successive years, it may be proper to give a detail of these last, and to incorporate them with the former so as to obtain a general average for the whole period of 25 years.

The rain-guage has been all the time situate in a garden on the S.E. side of Manchester; it is 20 yards distant from any house or elevated object that can influence the fall of the rain. The gauge is a funnel of 10 inches diameter, and the top is surrounded by a perpendicular rim of three inches high to prevent any loss by the spray; it is fixed in a proper frame with a bottle for the water, and it stands above two feet above ground.

Table of the Fall of Rain in Manchester.

Years.	1802.	1803.	1804.	1805.	1806.	1807.	1808.	1809.	1810.	1811.	1812.	1813.	1814.	1815.	1816.	1817.	1818.	Means.
	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
Jan	1·967	1·654	2·700	2·180	5·551	2·340	2·676	0·800		2·116	1·500	1·500	0·948	1·150	3·528	1·284	5·520	2·357
Feb.....	3·728	2·664	1·468	2·988	1·604	3·220	1·512	1·650		3·118	3·760	2·500	1·254	2·984	1·828	4·354	3·458	2·631
March...	0·860	2·589	2·037	2·450	1·060		0·180	0·444	2·000	3·566	4·584	1·154	1·440	4·132	2·830	2·190	5·026	2·283
April ...	1·972	1·333	3·060	1·134	0·500	2·976	1·982	0·994	1·368	1·738	1·270	1·818	2·864	0·944	2·318	0·170	3·154	1·663
May	0·860	2·496	1·364	2·164	1·114	2·160	2·702	3·366	0·948	5·136	3·022	5·574	0·300	3·966	3·538	2·702	1·188	2·653
June....	3·120	3·534	1·802	2·626	1·400	2·267	2·078	3·083	2·208	1·676	3·018	1·698	2·076	1·734	2·762	4·044	2·072	2·417
July.....	6·233	1·105	3·170	3·663	1·970	2·512	2·012	2·152	2·980	2·812	4·415	3·208	2·351	3·100	6·888	6·468	2·134	3·349
August..	3·221	1·728	2·890	2·918	2·904	5·806	3·828	4·656	4·082	3·508	1·058	1·932	4·490	4·646	1·184	5·507	1·088	3·109
Sept.....	2·176	1·264	0·240	1·460	2·006	2·216	2·702	5·300	2·076	0·320	2·682	3·060	1·158	3·116	2·926	1·313	3·718	2·431
Oct.	6·126	1·398	6·023	1·269	1·944	5·000	5·648	0·760	5·091	5·270	4·942	5·304	5·368	3·182	4·236	0·604	3·292	3·628
Nov.	1·882	3·749	3·064	0·624	5·858	2·710	3·628	2·600	4·074	3·928	2·304	4·940	3·756	3·426	3·496	2·516	4·526	3·490
Dec.	3·592	3·958	1·492	4·064	7·284		2·208		6·234	3·528	1·+	1·140	5·332	5·126	4·051	5·260	0·390	3·585
Total	35·737	27·472	29·310	27·542	33·465		31·156			37·726	34·±	33·828	31·340	37·506	40·285	36·412	34·566	33·596

Now, by blending these 17 years with the 8 years before referred to, we obtain the mean monthly averages for 25 years, as under; namely,

	Inches.
January	2·258
February	2·507
March	2·112
April	1·915
May	2·698
June	2·206
July	3·400
August	3·307
September	2·984
October	3·734
November	3·378
December	3·369
Total	33·868

I think there is reason to believe that if we had the averages for a century, they would not be materially different from these, either in regard to the relative monthly quantities, or to the annual quantity.

It may be proper, however, to observe, that the late Mr. George Walker, of Salford, gives an account of the rain at Manchester, for the nine years immediately preceding the above period of 25 years, in the Memoirs, vol. iv. p. 585. His observations have been incorporated with mine so as to extend the period from a quarter to a third of a century; but I prefer having his results separate, for the following reasons. On a comparison of our results for eight subsequent years, I found his average exceeded mine by about four inches in the year. (See Memoirs, vol. v. p. 668.) On inspecting his gauge, I had reason to think his mode of measuring the rain was not susceptible of sufficient accuracy, and suggested the same to him, with which he seemed to acquiesce. Besides this, the year 1792 (one of the eight) was a most remarkable one, in the north of England particularly. The annual depth exceeded the average amazingly; and it was occasioned by an excess in two or three of the months chiefly. The rain at Kendal that year was nearly 85 inches; and it was nearly the same at Keswick. April produced 10 inches, September 11 inches, and December 12 inches. Mr. Walker's rain in Manchester that year was 55¼ inches, which is far above the average; and nearly one half of this great quantity fell in three months; namely, May, September, and December. The year 1789 was also unusually wet. These facts influence the annual and monthly averages of Mr. Walker materially, independently of any supposed error in the actual measurement.

1

Averages of Mr. G. Walker's Account of Rain in Salford (Manchester) from 1786 to 1793 inclusive.

	Inch.	Incorporated with mine.
January	2·47	2·310
February	2·75	2·568
March	2·05	2·098
April	2·30	2·010
May	3·51	2·895
June	3·30	2·502
July	4·62	3·697
August	4·78	3·665
September	4·21	3·281
October	4·51	3·922
November	3·30	3·360
December	5·28	3·832
Total	43·08	36·140

Whether we consider the averages as deduced from Mr. Walker's observations, or from my own, or from the two united, the conclusion is equally obvious ; namely, that the first six months of the year must be considered as *dry* months, and the last six months of the year as *wet* months ; also, that April is the driest month in the year, and that the sixth after, or October, is the wettest, or that in which the most rain falls, in a long continued series of years, in the immediate neighbourhood of Manchester.

It would be interesting to inquire how far these conclusions apply to Great Britain in general, or to Europe at large, or still more generally to the northern temperate zone.

In the 4th vol. of the Society's Memoirs, p. 576, is given an abstract or summary of Mr. Hutchinson's account of rain at Liverpool for 18 successive years ; namely, from 1775 to 1792 inclusive. The annual average is 34·4 inches. Every one of the first six months yielded less rain (on the average) than any one of the last six months of the year. March was the driest, and October the wettest month in the year.

In the same volume, p. 580, there are given the results of 16 years' observations of the rain at Dumfries, by Mr. Copland, namely, from 1777 to 1793. The annual average was 37 inches. The driest month is April, and next to it March ; the wettest is September, and next to it October; and each of the first six months of the year is drier than any one of the last six.

At Chatsworth, Derbyshire, from the same volume, p. 586, *et seq.* I deduce the following averages for 16 years (1777 to 1792 inclusive), as per table.

Here again we see that March is the driest, October the

wettest, and all the former six months drier than any one of the latter.

By combining the 10 years' observations of Dr. Campbell, of Lancaster (Memoirs, iv. p. 264 and 591), we obtain similar results nearly. March is the driest, and August the wettest month at Lancaster. But 10 years is too short a period to obtain true means. I have the rain at Lancaster for a subsequent period of 10 years (1802—1811), furnished me by my friend John Ford, jun. Esq. of Ellel; which likewise gives March for the driest, but October for the wettest month of the year.

In the Annales de Chimie et de Physique (vol. viii. 1818), there is an account of rain at Viviers, lat. 44° 29′ N. long. 2° 2′ E. of Paris, by M. Flaugerges. The monthly means for 40 years' observations (from 1777 to 1818) are stated, from which it appears that February is the driest month in the year, and October the wettest. The annual average is 34 inches (French). The year 1801 was the wettest in that period, yielding 48 inches (French), and 1779 was the driest, yielding 20 inches 7 lines. Viviers, which is in the S.E. of France, has, however, some essential differences from Great Britain in regard to rain. There the months of July and August are among the driest; the only months distinguished for heavy rain are September, October, and November; while April and May yield each more than the monthly average.

I have collected the Royal Society's account of rain at London for 28 years, ending in 1806, and find the averages to stand as under; also those of Luke Howard, Esq. for a subsequent period of 12 years, ending with 1818, made in the vicinity of London. These united are as per table.

The fall of rain at Kendal for five years (1788—1792) was published in my Meteorology; since that time the account has been continued by my brother for 18 years, with which he has favoured me; I have obtained also two years further from good authority, making in all 25 years' rain. The monthly averages I have deduced as per table.

I deduced the average rain at Paris from observations published in the Journal de Physique for the last 15 years, as per table.

The average rain at Glasgow for 17 years (1801—1818) was deduced from a paper in the *Annals of Philosophy*, vol. xii. p. 377.

Mean Monthly and Annual Quantities of Rain at various Places, being the Averages for many Years.

	Manchester. 33 years.	Liverpool. 18 years.	Chatsworth. 16 years.	Lancaster. 20 years.	Kendal. 25 years.	Dumfries. 16 years.	Glasgow. 17 years.	London. 40 years.	Paris. 15 years.	Viviers. 40 years.	General average.
	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Fr. In.	Fr. In.	Inch
Jan.	2·510	2·177	2·196	3·461	5 299	3·095	1·595	1·464	1·228	2·477	2·530
Feb.	2·568	1·847	1·652	2·995	5·126	2·837	1·741	1·250	1·232	1 700	2·295
March...	2·098	1·523	1·322	1·753	3·151	2·164	1·184	1·172	1·190	1·927	1·748
April....	2·010	2·104	2·078	2·180	2·986	2·017	0·979	1·279	1·185	2·686	1·950
May	2·895	2·573	2·118	2·460	3·480	2·568	1·641	1·636	1·767	2 931	2·407
June	2·502	2·816	2·286	2·512	2·722	2·974	1·343	1·738	1·697	2·562	2·515
July	3·697	3·663	3·006	4·140	4·959	3·256	2·303	2·448	1·800	1·882	3·115
Aug.	3·665	3·311	2·435	4·581	5·039	3·199	2·746	1·807	1·900	2·347	3·103
Sept.....	3·281	3·654	2·289	3·751	4·874	4·350	1·617	1·842	1·550	4·140	3·135
Oct......	3·922	3·724	3·079	4·151	5 439	4·143	2·297	2·092	1·780	4·741	3·567
Nov......	3 360	3·441	2·634	3·775	4·785	3·174	1·904	2·222	1·720	4·187	3·190
Dec.	3·832	3·288	2·569	3·955	6·084	3·142	1·981	1·736	1·600	2·397	3·058
	36·140	34·118	27·664	39·714	53·944	36·919	21·331	20·686	18·649	33·977	

Observations on the Theory of Rain.

Every one must have noticed an obvious connexion between heat and the vapour in the atmosphere. Heat promotes evaporation, and contributes to retain the vapour when in the atmosphere, and cold precipitates or condenses the vapour. But these facts do not explain the phenomenon of rain, which is as frequently attended with an increase as with a diminution of the temperature of the atmosphere.

The late Dr. Hutton, of Edinburgh, was, I conceive, the first person who published a correct notion of the cause of rain. (See Edin. Trans. vol. i. and ii. and Hutton's Dissertations, &c.) Without deciding whether vapour be simply expanded by heat, and diffused through the atmosphere, or chemically combined with it, he maintained from the phenomena that the quantity of vapour capable of entering into the air increases in a greater ratio than the temperature; and hence he fairly infers, that whenever two volumes of air of different temperatures are mixed together, each being previously saturated with vapour, a precipitation of a portion of vapour must ensue, in consequence of the *mean* temperature not being able to support the *mean* quantity of vapour.

This explanation may be well illustrated by contemplating a curve, convex towards its axis, in which case the ordinates increase in a greater ratio than the abscissæ. The abscissæ represent temperature, and the ordinates the quantity of steam which the corresponding temperatures are capable of retaining.

In 1793 I published my Meteorological Observations and

Essays, a few years after this theory of rain had been made known; as far as I was then acquainted with it from one of the Reviews, it appeared the most plausible of any I had seen; but on looking at my remarks, it is evident I had not been made acquainted with its distinguishing feature, and that on which its excellence depends; namely, a higher *solvent power* (if it may be so called) in the air, than what is proportionate to the increase of temperature; and that the precipitation of vapour in the form of clouds and rain is occasioned not by mere cold, but a mixture of comparatively warm and cold air.

At the time of my publication of the Essay on Rain, &c. I had a strong bias to the opinion, that the steam or vapour in the atmosphere exists in a state of combination with heat, but without any chemical union with the elements of the atmosphere; only it is subject to be wafted along mechanically by the great body of the atmosphere in its ordinary currents. This opinion was founded and supported on the authority of the late M. Saussure in part; he having determined by direct experiment that a cubic foot of dry air of the temperature of 66° would imbibe 12 grains of water for its saturation. Now, from experiments on the boiling of water in vacuo, I was persuaded that this quantity of vapour was nearly what would fill a cubic foot of empty space, in the temperature of 66°; and, by analogy, I concluded that the quantity of steam necessary to saturate any given volume of air at any temperature was the same that would be requisite to fill an equal void space at the same temperature. This reasoning was of course hypothetical at that time, and unsupported by any direct experiment.

In 1801 a series of essays of mine were read before the Society, and subsequently published in the fifth volume of Memoirs; one object of experimental inquiry was, whether steam of any kind was the same in quantity in air and in a vacuum, all other circumstances being the same. The result was decidedly for the affirmative.

Another object was to ascertain the true force of steam in all atmospheric temperatures. This was clearly proved to be progressively increasing with the temperature, as Dr. Hutton had rightly conjectured. Indeed with a slight modification of the thermometrical scale, the temperature is an *arithmetical* progression, and the force of steam a *geometrical* one. Hence the curve showing the force of steam is what mathematicians call the *logarithmic*, one remarkably convex to its axis.

The cause of rain, therefore, is now, I consider, no longer an object of doubt. If two masses of air of unequal temperatures, by the ordinary currents of the winds, are intermixed, when saturated with vapour, a precipitation ensues. If the masses are under saturation, then less precipitation takes place, or none at all, according to the degree. Also the warmer the air, the greater is the quantity of vapour precipitated in like circum-

stances, as is evident to any one, on inspecting the logarithmic curve, or on considering that the increments of a geometrical progression, are in proportion to the terms. Hence the reason why rains are heavier in summer than winter, and in warm countries than in cold.

We may now inquire into the cause why less rain falls in the first six months of the year than in the last six months. The whole quantity of water in the atmosphere in January is usually about three inches, as appears from the dew point, which is then about 32°. Now the force of vapour at that temperature is 0·2 of an inch of mercury, which is equal to 2·8 or three inches of water. The dew point in July is usually about 58° or 59°, corresponding to 0·5 of an inch of mercury, which is equal to seven inches of water; the difference is four inches of water, which the atmosphere then contains more than in the former month. Hence, supposing the usual intermixture of currents of air in both the intervening periods to be the same, the rain ought to be four inches less in the former period of the year than the average, and four inches more in the latter period, making a difference of eight inches between the two periods, which nearly accords with the preceding observations.

In the preceding estimations of the whole quantity of water in the incumbent atmosphere of any place, I take for granted that an atmosphere of steam is blended with the general atmosphere throughout, in the same vertical column, and subject to the common law of rarefaction in ascending. This is a view of the aqueous atmosphere, which no one seems to have entertained but myself. I have been making experiments almost annually on the subject since 1802, on the mountains in the north of England, and particularly on Helvellyn. These experiments have been materially facilitated of late years by masses of snow, which have been found near the summit, in the month of July; but it has often happened that the cold springs of water near the summit have been adequate. By one or other of these, the dew-point of the air may be found at any required elevation on the mountain, and the law by which it is regulated in the ascent may be investigated. On some future occasion, I intend to draw up a memoir on this subject. In the mean time I may observe, that all the phenomena concur in exhibiting the same variation of density in the aqueous vapour atmosphere in its present mixed state, as would no doubt be observed in an atmosphere of pure steam of equal density.

ARTICLE III.

Experiments for a new Theory of Vision. By Dr. Joseph Reade.

(To Dr. Thomson.)

SIR,   *Cork, Dec.* 1, 1819.

Καλλιστα βιω. Xen.

PERHAPS no subject in natural philosophy has more engaged the attention of the learned, nor claimed more interest, than that concerning the proper seat of vision. For 200 years the retinal theory has been maintained, and its difficulties, if not absurdities, softened down by the learned ingenuity of mathematicians and metaphysicians, well aware that to overturn a theory so universally adopted, and stamped with the seal of antiquity, requires a number of well regulated experiments and legitimate deductions, I now commit my endeavours to the candour of the intelligent reader.

Experiment 1.—Having often remarked, when examining the eyes of patients, that surrounding objects, such as a lighted candle, &c. were painted on the transparent cornea in a beautiful and minute manner, as on the face of a convex speculum, it occurred to me that the mind might receive impressions or ideas from those erect images ; and I was the more desirous of bringing this interesting suggestion to the test of experiment in consequence of the many difficulties attached to the present system of vision. I now pasted two narrow strips of black cloth in the shape of the letter T, and about three inches in length, on one of the upper panes of a large and well lighted window. I then requested a gentleman with a large pupil and good sight to seat himself about four or five feet from the letter, and to fix his eyes steadily on it. Looking into his pupil I perceived the letter T to be minutely yet distinctly painted by reflection. I then took a plano-convex lens in my right hand, such as school boys use for burning glasses, and held it close to the pupil. On again looking at the corneal image of the letter T, I perceived it enlarged, or magnified, in all its dimensions, and the spectator said he also perceived it much larger than with the naked eye. On removing the lens a little further from his eye, I perceived the letter on the pupil not only magnified, but surrounded with colours, and the spectator saw the letter large, confused, and surrounded with colours. So far the phenomena of vision answered exactly to the changes of this corneal image. I next removed the lens somewhat further from the eye; and on looking into it perceived the letter T to be inverted, and the spectator likewise saw it inverted. He now took the lens in his own hand, and placing it at different distances before his eye, I was enabled, by means of

the corneal image, to tell him what he saw. Having again requested the spectator to fix his eye on the letter, I placed a double concave lens before the pupil, and the letter was immediately diminished; he now said he saw it very small. Here I shall beg leave to remark that these experiments strike at the very first principles as laid down for optical instruments. For we find by two simple and conclusive experiments, that a convex lens, instead of converging the rays, as first maintained by Maurolycus in his treatise "De Lumine et Umbra," actually and bonà fide diverges and magnifies the image in all its dimensions; and, on the other hand, that a concave glass converges or diminishes the image. The object of this paper being merely to draw the attention of the scientific to my opinions on optics, and particularly on vision, I shall not at present enter more fully into the theory of spectacles, &c.

Experiment 2.—Having placed a plano-convex lens at such a distance before the spectator's eye, as to form an inverted image of the letter T on his pupil, I placed a double concave lens behind to represent an opera glass, or gallilean telescope. The inverted corneal image immediately became erect, and the spectator said he also saw it erect.

Experiment 3.—The above experiments were made at about four feet from the window; I now requested the spectator to remove his chair to within a foot of the object, and on placing a convex lens immediately before the eye, the corneal image was considerably magnified: on slowly removing the lens nearer the letter, and further from the eye, the black corneal image began to be surrounded with colours, but did not become inverted, nor did the spectator perceive any change of position: when close to the object, the corneal image appeared better defined and more distinct. I now placed a prism before his eye, and desired him to look through the lower refracting angle; as he was unaccustomed to the application of this instrument, he could not regulate it so as to perceive the coloured image of the letter T. I, therefore, turned the prism until I perceived it on the pupil, and then told him exactly what he saw, making a mirror of his eye. Let us now inquire what changes the intervention of a plano-convex, or a concave glass, would make on the letter T brought to a focus on the retina by means of the crystalline and other humours. Having removed the fat and coats from the back part of an ox's eye, as performed by Kepler and Scheiner, and thus laid bare the retina, I placed a lighted candle in front. An inverted image was seen as if floating on the retina. I now placed a plano-convex lens between the candle and the cornea at such a distance as to form an inverted image on the pupil. The retinal image remained inverted. On now placing a double concave lens at a little distance before the convex one, the corneal and inverted image became erect, while the retinal inverted image

was not in the least changed as to position. . Here is a direct
experimental proof that even if an inverted image were painted
on the retina, that inverted image, not undergoing any change
of position by the intervention of the glasses, could not be the
image conveyed to the mind.   To suppose for one moment that
an inverted image on the retina could produce both the idea of
inversion and erection would be adding another inconsistency to
Kepler's catalogue.   In this experiment the changes of the
corneal image were accompanied by simultaneous changes in
the mind; therefore, that, and that alone, must have produced
the sensations.   On placing a glass globe about two inches dia-
meter, filled with water, opposite the letter T, and then inter-
posing a convex lens, the posterior inverted image was obliterated;
the rays of black light not being sufficiently strong, the same
thing took place with a concave lens.   Dr. Priestley, who wrote
a number of metaphysical works, gravely informs his readers
" that the want of an inverted image might produce the sensa-
tion of an erect one."   With the highest respect for the Doctor's
opinions, we might just as readily believe that the want of a
man's dinner would get him a supper !

There is no inverted Image ever painted on the Retina.—
Having removed the fat and coats from the back part of an
ox's eye, and thus bared the retina in imitation of Kepler's
and Scheiner's experiments, I placed a lighted candle on a table
in front; and on looking through the retina, my eyes being placed
beyond the principal focus of the sphere (or rather two segments
of one), I certainly did perceive a beautiful inverted image of the
candle, as if floating on the retina.   So far the experiment
seemed to accord with the retinal· theory of vision; for if the
rays were refracted and converged, as represented by optical
writers, by means of the cornea, aqueous humour, crystalline
lens, and vitreous humour, they should cross nearly in the centre
of the eye, and finally paint an inverted image on the retina.
However, on approaching my eye nearer to the retina, I per-
ceived the inverted image to become large, confused, and when
my eye was very close, it opened into two curved and inverted
images, which receded laterally ; and at a yet nearer approach,
formed into a circle, through the centre of which I perceived a
very distinct and erect image of the candle, evidently coming
from the anterior surface of the eye, and perfectly distinct from
the inverted one, considerably magnified in passing through the
humours.   Kepler, in placing his eye beyond the focus of the
ox's eye, which is nothing more than a simple sphere, saw an
inverted image, formed by the junction of the two images painted
on his own cornea, which he mistook for one on the retina, as a
person looking into a concave mirror thinks that he sees an
inverted image in the glass.   Here I think it necessary to give
a rough sketch of the passage of the ranys through the eye,

particularly as my opinions are diametrically opposite to those of
all optical writers.　A glass globe filled
with water, and about two inches dia-
meter, may serve those unacquainted
with morbid dissections.　 *a b,* two
rays of light coming from the upper
and lower parts of the candle, impinge
on the transparent cornea at *c,* and paint an erect image.　This
image again transmits rays, diverging as they pass through the
sphere to *g,* where the spectator sees a magnified and erect
image.　The image at the cornea *c* also sends rays forming
inverted images, in consequence of the rays crossing at *d* and *e.*
These images take the curvature of the globe, and uniting into
one inverted image, form what has been denominated the
principal refracted focus at *f.*　Now it is evident that Kepler, to
have made his experiments correctly, should have placed his eye
at *g,* and not at *f.*　Indeed his eye should almost touch the
retina; and then, as I have already said, he would have seen
an erect magnified image of the candle, and not an inverted one,
surrounded by what optical writers denominate a circle of aber-
ration.　It is really surprising how any person could for a moment
believe that this circle of aberration could produce vision.
According to the present theory of vision, long-sightedness they
say is produced by the image being formed beyond the retina,
short-sightedness by an image formed before the retina in the
vitreous humour—both physical impossibilities.　The rays of light
are supposed to be converged in the body of the eye.　I would
beg leave to put the following question : Would the crystalline
lens when imbedded in the vitreous humour act in the same man-
ner as it would in air ? Certainly not, as the following easy
experiment may show.　Take a large basin of water, hold a
powerful glass lens in such a position over the water as to form
an inverted image on the side of the basin and under the water,
then immerse the entire lens, and at no distance can a focus be
ever formed, the circular shadow with its black circumference is
perceived, but nothing else.　Now surely the refractive power
of glass, in proportion to that of water, is much greater than that
of the crystalline lens in proportion to that of the vitreous
humour.　Mr. Harris, in his Optics, p. 95, says, " It is very
difficult, I think, to determine accurately the measures of these
refractions ; but, from such experiments as could be made, it
has been found that the refractive powers of the aqueous and
vitreous humours are each of them much the same with that of
common water, and that of the crystalline is a little greater :
that is, the proportion between the sines of incidence and refrac-
tion out of air into the cornea, or aqueous humour, is as 4 to 3,
out of the aqueous humour into the crystalline as 13 to 12 ; and
out of the crystalline humour into the vitreous as 12 to 13."
From analogy we are authorised to conclude, that the crystalline

lens can never form inverted images on the retina, and that the
lens is placed in the centre of the eye to magnify or diverge the
rays, and not to invert the object. Moreover, that the crystal-
line lens does not produce inverted images on the retina is shown
by what takes place when removed by the operation of depres-
sion or extraction. For if the lens were so essentially necessary
to vision, its removal must cause blindness. In answer it has
been said, that after the operation, the patient is obliged to use
convex glasses, or spectacles, to supply the place of the lens.
From many years' practice in these complaints, I am enabled to
say, that this is by no means the case. In young patients, the
use of convex glasses, although at first of assistance, is ultimately
unnecessary, if not injurious, for as the eye gains strength, they are
enabled to see all objects at a limited distance fully as well as
those labouring under short-sightedness. Some time since I
removed a congenital cataract from the right eye of Mary Skil-
lington, aged 19. After the operation she never wore a glass,
and can now see to thread a needle ; she also sees perfectly
well at different distances to the extent of 200 feet and upwards.
Miss Jenkins, of Bantry, writes and reads perfectly well, and
attends to the business of her shop without the use of spectacles.
This Lady came to Cork to consult a London quack, who pro-
fessed to cure all diseases of the eye that were curable: luckily
for this patient she did not come under the denomination. Indeed
after the operation in young subjects, I never recommend the
use of a convex glass. In those patients wanting the crystalline
lens, the rays cannot come to a focus on the retina; yet had
Kepler and Scheiner removed the lens from the ox's eye, as I
have repeatedly done, they would have found that it made not
the slightest difference in the inverted image, which they con-
ceived to float on the retina. Neither could the crystalline jump
backwards and forwards to accommodate the eye to the object
at different distances. Indeed I cannot conceive the cause of
this jumping of the lens. If the distance of the object be ascer-
tained, and consequently the object seen before the lens, with
its thousands and tens of thousands of muscles, begins to jump,
what occasion is there for that movement? But if the jump take
place before the object be seen, then the extent of the jump can-
not be ascertained. Look before you leap should be a maxim
with all metaphysical jumpers. The fact is, that the eye princi-
pally judges of different distances by comparing the visible size
of the corneal image with the educated sense of the tangible
object, intervening objects, strength of colouring, &c. On view-
ing a painting, the objects are all equidistant on the canvass ;
yet we conceive them to be at relative distances. The Supreme
Being, with an invisible hand, paints the images of external
objects on the corneal canvass, and the mind conceives them to
be at relative distances on the same principle. The following
easy experiments may also show that the rays diverge in passing

through a sphere, or convex glass. Take a cylindrical tumbler, fill it with clear water, and hold it in the left hand opposite a window. Hold a black slate pencil, or any other slender body, about three inches in length behind this glass vessel; when close, one magnified image is seen, but on gently withdrawing the pencil to a greater distance, this image becomes more magnified, and at a certain distance, two images, fully as well defined, are seen at each side of the tumbler; on continuing to withdraw the pencil two everted images are seen to glide with a considerable degree of curvature towards the posterior surface of the tumbler and at last coalesce into one image, which obliterates the anterior one, or that formed at the anterior surface. This corroborates the inferences drawn from the former experiment. When the object is near the posterior surface of the tumbler, the eye receives the rays considerably diverged or magnified; when the object is at some distance from the posterior surface, the eye receives the rays from the coalesced image formed from the two lateral images. From this experiment there can be no doubt whatsoever that the eye receives rays from two distinct and separate images; and also that the mind receives impressions from a glass globe or convex lens in nearly a similar manner. Should a doubt yet remain, the following experiment may be made : Place a red wafer under one of the planes of a triangular glass prism, resting on a sheet of white paper; we immediately see two everted images of the wafer formed in each lateral plane,

as thus represented . The wafer *a* sends rays or images

to *b* and *c*. As the prism has plane sides, the two images can never come to a focus at any distance; but if we round off the angle, they immediately unite, and form an oblong image of the

wafer, as thus represented .

From these experiments, and many others hereafter to be related, in the second volume of the Experimental Outlines, not a doubt remained on my mind that reflected erect, and not inverted images, gave mental impressions of a visible world. Surely if any thing can increase our admiration of the power and wisdom of a Supreme Being, it is the conviction that a beautiful and ever varying landscape is painted in miniature on the transparent cornea. When we consider that the black choroid shines through the retina, we should admit that it is very unfit to be the reflecting mirror of the mind. To bring this to the test of experiment, I turned out the aqueous, vitreous, and crystalline humour of the eye of an ox; on bringing the inverted image of the black letter T pasted on the window to float on the retina, by means of a convex lens, I found that it was perfectly invisible; in some places confused; indistinct in all. Indeed the retina, were it free from this and many other objections, and also free from

the large blood-vessels and nerve running over its surface, from its being of a grey colour, like pounded glass or animal jelly, would be very unfit, and could never form an image of a grey object perfectly similar to itself, neither could objects the colour of the choroid coat ever be seen. We might as well think of writing with black ink on a sheet of black paper, as attempt the formation of dark images on a dark ground. On the other hand, how admirably fitted both for reflection and transmission is the cornea, both sufficiently transparent and sufficiently opaque; no coloured substance could answer the purpose. It has hitherto been the received opinion, that the two optic axes, concurring at the object, make an angle, according to the size of which the object appears large or small; but this opinion, whose inconsistency has been already pointed out by Bishop Berkeley, must yield to the more rational theory, that the mind takes the apparent magnitude and distance from the size of the corneal image, and not from lines and angles beyond the nervous influence, or from invisible rays, all rays being invisible, which are transparent until intercepted and reflected. " In vain (says Berkeley) shall all the mathematicians in the world tell me that I perceive certain lines and angles which introduce into my mind the various ideas of distance, so long as I myself am conscious of no such thing." Indeed we might as well believe in ghosts and hobgoblins as believe that we could see an object, or the image of an object beyond the nerves; that is, beyond the transparent cornea. Here is the rubicon, the utmost limit beyond which the mind can never travel. Surrounding objects are brought to the eye by means of the rays of light: hence the nerves convey them to the sensorium. Indeed the idea that the mind could travel beyond the cornea, ride on the whirlwind, and, like a fairy mab, measure invisible angles of an invisible and distant image, is so very inconsistent that we cannot but express surprise at its adoption. If a man were gravely to say that he could touch the moon, he would be looked on as mad; but an astronomer says, that on looking through a telescope he can measure the invisible image of that body nearer to the eye than the moon, and beyond the influence of the nerves; and the astronomer gets credit for the assertion. As the knowledge of distance almost entirely arises from experience, founded on the analogy between the sense of sight and touch, the former at a very early period of existence is inadequate to regulate our perceptions. When an infant begins to notice, natural education commences, external objects are the letters, and the nerves the instructors of the mind. The insufficiency of sight is evident by the anxious desire to feel and to examine every new plaything. The image of the rattle is delineated on the cornea, and the child believing it to touch the eye, grasps at it although far removed. On the same principle I have heard a child cry bitterly for the moon to play with. In a few months, the sense of touch has partly educated the eye in judging dis-

tance by the apparent magnitude of the corneal image. A man
born blind and suddenly restored to sight would suppose every
object to touch his eye. All that is accomplished by telescopes
and microscopes (according to the retinal theory) is first to make
an image of a distant object by means of a lens, and then to give
the eye some assistance for viewing that image as near as possible;
so that the angle which it shall subtend at the eye may be very
large, compared with the angle which the object itself would sub-
tend in the same situation; this is done by means of an eye-glass
which so refracts the pencils of rays that they may afterwards
be brought to their several foci by the natural humours of the
eye. Now it is evident from the foregoing experiments that this
theory is perfectly erroneous, and that a telescope, as shall here-
after be more fully shown, does nothing more than diverge the
rays, or magnify the image on the cornea. In the gallilean
telescope, the convex lens magnifies the erect image which it
forms on the concave eye-glass, the use of which, by regulating
the sphere of concavity, is to obviate the colours produced by
the sphere of convexity. Hence an achromatic and magnified
corneal image is formed. I shall here notice a difficulty which
Dr. Barrow and all other opticians have failed to clear up, parti-
cularly noticed by the Bishop of Cloyne. " Let an object be
placed beyond the focus of a convex lens, and if the eye be close
to the lens, it will appear confused, but very near to its true
place. If the eye be a little withdrawn, the confusion will
increase, and the object will seem to come nearer; and when the
eye is very near the focus, the confusion will be exceedingly
great, and the object will seem to be close to the eye. But in
this experiment the eye receives no rays but those that are con-
verging; and the point from which they issue is so far from being
nearer than the object, that it is beyond it; notwithstanding
which the object is conceived to be much nearer than it is,
though no very distinct idea can be formed of its precise dis-
tance." Here Dr. Barrow supposed that when his eye was close
to the lens it received none but converging rays; whereas, on
the contrary, they were diverging; and as he withdrew his eye,
the more the erect image was magnified, when magnified beyond
the standard of distinct vision it became confused. But when
the eye was beyond the focus, the anterior or erect image was
lost to the eye, and the two lateral and inverted images coalesc-
ing into one, formed an image which was nearer the eye floating
as if on the posterior surface of the lens. Dr. Barrow, like a
true philosopher, acknowledges himself unable to account for
this appearance, finishing his lecture with this observation:
" Vobis itaque nodum hunc, utinam feliciore conatu, resolvendum
committo." Whether these experiments tend to untying the
knot, I leave the reader to determine, and shall not enter on
Berkeley's or Barrow's theories of apparent distance in this
paper.

We next come to the rectification of inverted images on the retina. This, according to Scheiner and Kepler, is the business of the mind, which, when it perceives an impression on the lower part of the retina, considers it as made by rays proceeding from the higher parts of the object tracing the rays back to the pupil where they cross one another. But this hypothesis (says Dr. Priestley, a great metaphysician) will hardly be deemed satisfactory; and, by way of clearing up the difficulty, he proceeds thus : " Upper and lower are only relative terms; and as all objects are painted upon the retina in a similar manner (all the upper parts in one direction, and all the lower parts in the other), it is by custom only, founded on experience and the association of ideas, that we learn to distinguish them from one another, so as to direct our eyes, or point our hands upwards or downwards, as we have occasion. If this be the true solution (continues the learned Doctor) it will follow that if the images of objects had always been painted in a different manner, that is, erect as the objects themselves are, we should have acted as we do now without being sensible of the difference, a different association of ideas only having taken place." Now all this laboured explanation comes to nothing more or less than that we are taught by experience. However, we never find the infant or the brute (incapable of these refined associations) mistaking the top for the botttom, or the right for the left. When the world was turned upside down by philosophers, they should have attributed the circumstance to blind instinct, and not to reason. For indeed reason has nothing whatever to do with the business. In the summer of 1812, I performed the operation for cataract on a very intelligent boy, named Edward Carey, aged 10 years ; he was born with such opaque cataracts as merely to enable him to distinguish light from darkness, or the shadow of an interposed hand, but was incapable of distinguishing the outlines of any object, or the most brilliant colours. After the operation, and before he could acquire any ideas from association, having inquired the manner in which he saw, he answered that he saw objects as he felt them, supposing them to be very near the eyes.

Although Cheseldon was an advocate for the retina being the seat of vision, he does not make any particular observations on this difficulty, but says that the young gentleman whom he couched with congenital cataracts " knew not the shape of any object, nor any thing from another, however different in shape or magnitude; but upon being told what things were, whose form he before knew from feeling, he would carefully observe, that he might know them again." When shown his father's picture and told what it was, he acknowledged a likeness, but did not mistake the head for the feet. Indeed were there no other difficulty in the retinal theory of vision, the inversion of objects, or turning the world upside down, and making confusion of right and left,

would be sufficient to invalidate the entire. The next difficulty in this catalogue of difficulties is the power of seeing objects distinctly at different distances. It is allowed on all hands that to see an object at different distances, either the retina or the crystalline lens must approach so as to shorten what is called the optic axis; so that the crystalline, according to this theory, must be a great jumper. To calculate the number of jumps, or miniature leaps, the lens of a general officer would take at a review, might puzzle an algebraist, vulgar arithmetic being perfectly inadequate to the solution. And then an able philosopher has given thousands and tens of thousands of muscles, or wings, if you please to call them so, to this little busy, fluttering thing. Indeed Dr. Young might as well have given muscles to an onion, the laminæ of which, and those of the crystalline, being very similar. On examining all the different theories, we find them all differing, and perfectly inadequate to the effect. Kepler, two centuries ago, supposed that the contraction of the ciliary processes draws the sides of the eye towards the crystalline, by which means the eye is lengthened, and the retina pushed to a greater distance from the pupils when we are viewing near objects. Mr. Thomas Young differs from Kepler, Descartes from Young, Haller from Young, with a crowd of others whose opinions I think it unnecessary to mention.

I remain, Sir, your obedient servant,

JOSEPH READE, M.D.

P.S. The Editor's observations would be acceptable.

ARTICLE IV.

Reply to Mr. Holt on Rain-Gauges. By Mr. Meikle.

(To Dr. Thomson.)

SIR,   *Berner's-street, Feb.* 23, 1820.

IN the *Annals of Philosophy* for October last, I gave a very concise but unobjectionable refutation of the mistaken idea which M. Flaugergues and others entertain about the true cause of the difference observed in the quantities of water collected in rain-gauges placed at different heights. I then flattered myself that, by means of a simple diagram, I had brought down the subject to the level of the most superficial inquirer; and, therefore, did not encumber your pages with a tiresome harangue about a thing so extremely simple and obvious to every one.

Some persons, however, seem still to cherish a predilection for their favourite error; and among these your learned correspondent Mr. Holt certainly holds no inferior place; since in your

number for January he has favoured us with an article in which he not only shows clearly that he has entirely misunderstood my explanation, but that he, if possible, still labours under a delusion superior to that of M. Flaugergues himself; who probably fell into that unaccountable mistake merely through haste, or from having his attention so much occupied with the many interesting services which he renders to science.

The Wernerian Natural History Society and the Bibliotheque Universelle are both of high authority; but an error is still an error in these, as well as truth is truth, should it occur in a novel. Unfortunately, however, for your correspondent, and the cause he has so faithfully espoused, the position I advanced is independent of authorities or opinions, since it must stand or fall with some of the simplest truths of geometry.

· Indeed it is almost inconceivable how any one who is but slightly acquainted with elementary geometry should feel the least embarrassed on seeing clearly that the *horizontal* distance, or distance of the points, in which the drops pass through a plane parallel to the horizon, is absolutely independent of their inclination where the wind runs steadily and horizontally. This I formerly showed, and shall now endeavour to do so again a little differently.

Let A C, B D, with the intermediate parallels, represent the paths of rain-drops falling perpendicularly; and let A E, B F, with the parallels between them, be the paths of the same drops when acted on by a steady wind blowing from B to A. Suppose A B and E D parallel to the horizon. Now since A C must be parallel to B D, and A E to B F, we have E F = A B = C D. A gauge, therefore, of the width C D would exactly receive the same quantity of rain if placed at E F, let the inclination be what it may. Consequently *the quantity of water received by a rain-gauge is totally independent of the general inclination of the rain.*
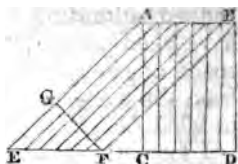
If F G be perpendicular to A E, it must no doubt be less than E F. But however short F G may become, all the drops still pass through it; since the parallels A E, B F, &c. are only so much the more crowded together. The grand principle, therefore, of M. Flaugergues's mistake is his always proceeding on the supposition that the shortest or perpendicular distance of the lines in which the rain falls is constant; whereas that varies with the sine of inclination; while it is the distance of the points in which the drops pass through a horizontal plane that is invariable. Mr. Holt has duly adopted the same mistake, only trying to improve upon it, by saying that the quantity of rain received will be proportional to the *angle* of inclination; whereas M. Flaugergues makes it as the *sine* of that angle.

As the rain drops, if first acted on by the wind, and afterward

gradually sheltered from it in approaching the ground, must
obviously descend in curves; let H I, K L,
with the curves between them, represent the
paths of the falling drops. Then since it is
evident these curves must all be every way
equal and similarly situated with respect to the
horizon, it follows, that any line H K parallel to
the horizon must be exactly equal any other parallel I L; so
that the general obliquity of descent was no concern whatever
with the quantity of rain which the gauge receives.
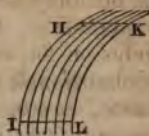
"Should the rain," says Mr. Holt, "be blown in a direction
parallel to the horizon, it is obvious *none* could enter." He
might easily have added abundance of other remarks equally true,
but like that altogether foreign to the point in question. Since
it is not difficult to show that no wind running parallel to the
horizon can ever carry rain drops also parallel to the horizon,
until the velocity of that wind become infinite; and when that
takes place, your correspondent would do well to look out for
the nearest place of shelter.

From what I have shown above, we may venture to conclude
that there is very little reason for constructing a particular sort
of gauge to counteract a source of error which does not exist.
Indeed admitting the opinion of these learned gentlemen to
have been correct, the quantity of rain which falls on any given
space of ground during wind would be less than what came away
from the same area of cloud. Query—Is the rest annihilated,
or what becomes of it that it does not reach the earth?

I formerly stated as my opinion that the paradox in question
was some way owing to the obstruction which the gauge itself
offers to the wind. This idea (the first of the kind I recollect to
have met with) has so highly pleased Mr. Holt, that he has con-
descended to give it in a slightly different form, as if it were
entirely new and his own.

Until some unexceptionable method be discovered for esti-
mating the error to which gauges in an exposed situation are
liable, I do not conceive such gauges are entitled to any notice
whatever; and since that error must be affected with the size of
the rain drops as well as with the velocity of the wind, I despair
of ever seeing the matter put to rights. But if Mr. Holt will be
so good as consider the subject with a little more attention, I
hope "it will appear to him that he himself has taken the wrong
view of it."

I am, Sir, your most obedient servant,
                                        HENRY MEIKLE.

ARTICLE V.

Observations upon the Ores which contain CADMIUM, *and upon the Discovery of this Metal in the Derbyshire Silicates and other Ores of Zinc.* By Edward Daniel Clarke, L.L.D. Professor of Mineralogy in the University of Cambridge, Member of the Royal Academy of Sciences at Berlin, &c. &c. In a Letter to the Editor of the *Annals of Philosophy*.

(To Dr. Thomson.)

DEAR SIR, *Cambridge, Feb. 18, 1820.*

IN vol. xiv. of your *Annals*, p. 269, you gave some new details respecting *Cadmium* from the " *Annalen der Physik*," by *M. Stromeyer*, which excited in my mind a very great desire to see the ores which are said to contain this curious metal. Some varieties of *radiated blende* from *Przibram*, in *Bohemia*, are described as containing two or three per cent. of *cadmium*. At a sale which took place soon afterwards in London, I procured specimens of the particular mineral thus alluded to, which were sold under the name of *splendent fibrous blende* from *Przibram*, pronounced *Pritzbram*. I found afterwards that they had been brought to England by Mr. J. Sowerby, of Lisle-street, a dealer in minerals, from whom I afterwards obtained more of the same substance. Upon my return to Cambridge, I endeavoured to obtain *Cadmium* from this ore, and succeeded, not following exactly the process mentioned by *M. Stromeyer*, because I made use of *muriatic acid*, in the first place, as a solvent, instead of the *sulphuric*, as being easier of evaporation; and hoping, by a careful evaporation to dryness, to separate any *lead* that might be present, the crystals of *muriate of lead* not being soluble in distilled water. Before any thing further is stated, it may be proper to describe the ore itself. The *splendent fibrous blende* of *Przibram*, in its external appearance, is not unlike red *hydrosulphuret of antimony*, but it is so highly splendent as to exhibit a lustre nearly metallic, especially after a fresh fracture. It exhibits shining fibres, as radii diverging from a common centre, imbedded in common massive *blende*, which also has something of a radiated structure, and is associated with an aggregation of cubic crystals of *sulphuret of lead*. The purer fibrous part of the mineral, divested of the massive *blende* and of the *sulphuret of lead*, was selected for experiment. Its specific gravity in distilled water, at a temperature equal to 55° of Fahrenheit, is exactly 4000. The Abbé Haüy makes that of *sulphuret of zinc*, or the common *blende*, to be equal to 4,1665.

(A.) Twenty-five grains of this mineral triturated in a porcelain mortar exhaled a strong smell of sulphuretted hydrogen simply

by fracture and friction. It filled the whole house. Placed in strong muriatic acid, and the acid boiled, a solution took place, but without any rapid or vehement action, sulphuretted hydrogen being evolved as before. The solution was then evaporated to dryness, and distilled water being added, the whole of the muriates were taken up, no *lead* being present for separation; but there remained undissolved a small portion of glittering heavy white particles, which, when collected on a filter and dried, weighed nine-tenths of a grain. These particles being examined with a lens were as diaphanous as the most limpid rock crystal. They proved to be diaphanous quartz in the arenaceous form: some of them had the rounded botryoïdal appearance of *Santilite;* others were angular and polygonal.

(B.) The filtered liquor collected from (A.) yielded an *orange* or *orpiment-coloured* precipitate to *sulphuretted hydrogen:* also a *white* precipitate to *hydrate* of *potass,* which was redissolved by adding *hydrate of ammonia.*

(C.) The same *orange-coloured* precipitate from (B.) being redissolved in *muriatic acid,* and the acid evaporated and distilled water added, *carbonate of ammonia* was poured into the solution in excess, which, holding the *zinc* in solution, threw down a *white* precipitate; and this changed *yellow* in drying by the loss of a portion of its carbonic acid.

(D.) The precipitate from (C.) being redissolved in *muriatic acid,* and the excess of acid driven off, and distilled water added as before, yielded an *orange-coloured* precipitate to *sulphuretted hydrogen,* distinguishing it from *zinc:* also to *phosphate of soda,* instead of the crystalline flakes or scales which *zinc* exhibits, it yielded a white pulverulent powder, which was redissolved in *liquid ammonia.* It was, therefore, *carbonate of cadmium.* It was moreover insoluble in water. But in drying the *muriate of cadmium,* if too much heat be applied, the salt is decomposed, and the *oxide* with a beautiful orange colour separates in an insoluble form simply by adding water.

A stick of *zinc* of a cylindrical form, being placed in the diluted muriate mentioned in (A.) became coated over with a precipitate which had a dendritic appearance. When examined with a lens, minute metallic scales of a leaden aspect were discernible. Having collected this precipitate by *zinc* into a watch-glass, and washed it, and evaporated the supernatant fluid, the residue appeared, a brown-coloured substance, which powerfully attracted moisture. Having exposed it almost to a red heat, it yet deliquesced in the instant of its cooling. Scraping off some of this on *platinum* foil, and heating it with the blow-pipe, it sent off white fumes; then intumesced, and exhibited a dark-brown, slag-like, substance, which, by further exposure to heat, was converted into an *orange-coloured oxide;* and this again by alternately applying the point of the blue or the yellow

flame, became, either a dark slag, or an *orange-coloured oxide*, until the whole was dissipated by increasing the temperature. The last globules of the dark slag among the *orange-coloured oxide* had a metallic appearance. When *borax* was added to the above, a fine amethyst colour appeared, while hot, which vanished when it became cold. To prove that this precipitate by *zinc* was really *cadmium*, I redissolved it in *muriatic acid*, and, after the usual process, *sulphuretted hydrogen* gave its beautiful *orange-coloured* precipitate, and *hydrate of potass* a *white* precipitate, as before; which became redissolved by adding liquid *ammonia*.

I have been the more particular in detailing these experiments, because upon their accuracy mainly depends the validity of my subsequent remarks respecting the presence of *Cadmium* in other ores of *zinc*. In the course of them, I had made some observations which led me to conclude that *Cadmium* is more remarkably characterized by a tendency to crystallization, than even *antimony*, or any other metallic body. When the *muriate of cadmium* is dissolved in a very considerable body of water, far below saturation, as it adheres to the sides of a glass vessel and becomes dry, it shoots out into transparent fibrous crystals, which radiate in a very beautiful manner. Many other solutions of the metal are moreover characterized by a tendency to crystallization. Having observed this, I began to suspect that a radiated structure in ores of *zinc* might, perhaps, be an indication of the presence of *Cadmium* in these ores; and accordingly I began the examination of a specimen of *silicate of zinc*, which I had brought from the same sale, and which had been described in the sale catalogue as *electric calamine* from *Freyberg*. It exhibited black diverging fibres, accompanied by an orange-coloured earth, and, perhaps, it may be the sort of *black fibrous blende* in which *Stromeyer* is said first to have discovered *Cadmium*.

Having dissolved a part of this mineral in *sulphuric acid*, and evaporated the acid almost to dryness, a sufficient quantity of distilled water was added to enable the fluid to pass the filters without destroying them; and having collected the clear liquor, *sulphuric acid* was again added, that an excess of the acid might be present according to *Stromeyer's* process. A stream of *sulphuretted hydrogen gas* was then sent through the solution, and immediately a most vivid *orange-coloured* precipitate began to fall. With this precipitate I repeated the experiments beforementioned, of solution in *muriatic acid*, &c. &c. and after the usual process obtained *carbonate of cadmium*, as before, having all the characters of that *carbonate*.

I made several attempts to revive the metal, but with little success. By heating the *carbonate* and expelling the acid, I obtained the *oxide of cadmium*, and by placing this in a glass tube containing *hydrogen gas*, and making the tube red-hot, a brilliant white-looking metallic appearance became fixed upon

the inner surface of the glass; but which may, perhaps, be due to *lead* contained in the glass itself.

At another time, having thrown down with *sulphuretted hydrogen* the *sulphuret of cadmium*, and observing that its fine *orange colour* was darkened by the presence of *lead*, I endeavoured to ascertain the quantity of *lead* present by reviving it in a crucible with *soda*. In this manner I obtained globules of pure *lead*, which had separated from other bronze-looking globules that were brittle, and these after solution, &c. in *muriatic acid* gave an *orange-coloured* precipitate to *sulphuretted hydrogen*, and a *white* precipitate to *hydrate of potass*, which was redissolved in liquid *ammonia*. It is evident, therefore, that they contained *Cadmium*, and were, perhaps, alloyed with *copper*: but the heat necessary for melting copper would volatilize *cadmium*, if *Stromeyer's* observation be correct, as stated in p. 274, vol. xiv. of your *Annals*.

Having now exhausted my materials, I went to London to see if I could procure any more of the dark *fibrous silicate of zinc*. At Mr. *Mawe's* shop in the Strand, I was shown something of a similar nature, although not agreeing as to colour. This was the *Derbyshire silicate of zinc*, having a greenish colour, with a radiated structure, like *wavellite*, and containing in cavities the *reddish-brown*, or *orange-coloured* earth before-mentioned. The specific gravity of the pure fibrous part equals 3·6767, but it is a very impure mineral, containing, as well as the preceding *silicate*, both *copper* and *iron*, besides a considerable quantity of *magnesian carbonate of lime and fluor spar*. Mr. *Mawe* furnished me with several of these specimens, and our professors of chemistry and geology, *Cumming* and *Sedgewick*, have kindly added more. In all of them I have found *Cadmium*: and the quantity of this metal in the ore may be, perhaps, ascertained; because 540 gr. of the mineral, by the process I have already described, yielded $3\frac{2}{10}$ths of *sulphuret of cadmium*, allowing rather less than $\frac{6}{10}$ths of a grain per cent. I have sent the *carbonate of cadmium* and the *sulphuret* to you for examination; and you have confirmed what I have said as to their real nature. The truth of the foregoing observations respecting the presence of *Cadmium* in our English ores of *zinc* has also been since confirmed by Dr. *Wollaston* and by Mr. *Children*, who have examined the Derbyshire *silicates* whence I obtained the metal, and obtained *Cadmium* from them.

Since making the foregoing observations, I began the examination of other English ores of *zinc*, and especially of an ochreous earthy-looking *carbonate of zinc*, from *Aldstone Moor*, in *Cumberland*. This ore is dug near the house of a dealer in minerals, of the name of *J. Cowper*, who resides in the town of *Aldstone*. After its solution in *sulphuric acid*, when a stream of *sulphuretted hydrogen gas* is sent through the solution, it assumes a vivid *orange* colour, and a precipitate is thrown down, which has the

colour of *rhubarb;* but this precipitate, when boiled in highly concentrated *muriatic acid,* is not wholly soluble in that acid; and when the excess of acid has been driven off, and distilled water added, no precipitate is afforded to *carbonate of ammonia.* *Zinc* throws down from it a dark precipitate, which has not the characters of *Cadmium.* Hence I conclude that it does not contain *Cadmium;* and that the *orange-coloured* precipitates afforded by *sulphuretted hydrogen* from the solutions of the ores of *zinc* are not of themselves indications of the presence of that metal, wanting the subsequent proof of its presence to which I have alluded; and the further testimony from subsequent tests used in the examination of the *carbonate;* as described in the foregoing experiments.

I have the honour to be, dear Sir, &c. &c.

E. D. CLARKE.

ARTICLE VI.

Experiments to determine the Composition of different inorganic Bodies which serve as a Basis to the Calculations relative to the Theory of Chemical Proportions. By J. Berzelius.

(*Continued from* p. 98.)

Barytes, Sulphate, and Muriate of Barytes.

TEN grammes of pure muriate of barytes perfectly deprived of water were dissolved in water, and the solution was mixed with nitrate of silver as long as any precipitate fell. I obtained in one experiment 13·806, and in another, 13·808 grammes of fused muriate of silver. Muriate of barytes, therefore, is composed of

Muriatic acid ...... 26·37 ...... 100·000
Barytes .... 73·63 ...... 279·226

If we calculate from this experiment the composition of barytes, we find that it must contain 10·45, per cent. of oxygen.
Ten grammes of muriate of barytes decomposed by sulphuric acid yielded in one experiment 11·217, and in another 11·218 grammes of sulphate of barytes. Hence sulphate of barytes is composed of

Sulphuric acid ...... 34·337 ...... 100·00
Barytes. .... 65·643 ...... 191·07

If we calculate the composition of barytes from these data, we find that it should contain 10·443 per cent. of oxygen. The results of these two experiments then only differ 0·00008, and may consequently be considered as very near the truth.

Composition of the Acids of Phosphorus, of the Phosphates, and
Phosphites.

The greater number of what is contained in this article having
been already published in the *Annals of Philosophy*, it will be
necessary to mention merely some new experiments which the
author has made on the phosphates of barytes and lime. He
had observed that the neutral phosphate of lime deviated a little
from the composition which ought to result from the general
capacity of saturation of phosphoric acid. He discovered after-
wards that this deviation was owing to the great tendency which
phosphoric acid has to produce the same subphosphate which
exists in the bones of animals, a greater or smaller quantity of
which always mixes itself with the neutral phosphate, when we
endeavour to procure this last. He found likewise that phos-
phate of barytes gives a subphosphate when treated with caustic
ammonia, and that in this subphosphate, the acid is combined
with $1\frac{1}{4}$ as much of base as in the neutral salt : that is to say,
that the salt is composed of

Phosphoric acid ...... 27·07 ...... 100·0
Barytes............. 72·93 ...... 269·3

The different anomalous subsalts and supersalts which phos-
phoric acid produces with barytes and lime, deserve the attention
of chemists ; and that so much the more because hitherto they
constitute the only examples of their kind.

When the quantity of phosphoric acid is the same, the multi-
ples of the two bases in their different combinations with the
acid are as follows ; beginning with the combination, which
contains the least base :

	Barytes.	Lime.
Biphosphate	1	1
Acid phosphate prepared with alcohol.	$1\frac{1}{4}$	$1\frac{1}{4}$
Neutral phosphate.	2	2
First subphosphate	$2\frac{1}{4}$	$2\frac{1}{4}$
Second subphosphate...............	3	3

If, on the other hand, the quantity of base remains the same,
the multiples of the acid are as follows, beginning with the sub-
salt, which contains the least acid :

	Barytes.	Lime.
Second subsalt.	1·0	1·000
First subsalt....................	1·2	1·125
Neutral salt....................	1·5	1·500
Acid phosphate prepared by alcohol	2·0	2·250
Biphosphate.	3·0	3·000

We see from this comparison that the anomalies fall only on the
intermediate combinations, to which we at present know nothing
analogous in the combinations of the other acids with the bases.

Experiments on the Composition of Boracic Acid.

With respect to this acid, we have direct experiments by Davy, and by Gay-Lussac and Thenard. The former found 73 per cent. of oxygen, while the latter found only 33 per cent. As it appeared to me very likely that the determination of the capacity of saturation of boracic acid would indicate which of these two results, so different from each other, approached nearest the truth, I undertook some experiments to determine that point. I heated boracic acid to redness in a platinum crucible to drive off the sulphuric acid with which it is usually contaminated. I dissolved the fused acid in boiling water, and crystallized it a second time. The crystals being well dried were exposed on a sand-bath to a heat above 212°; but long before becoming red-hot, they lost 0·221 of their weight. When heated in a platinum crucible by means of a spirit lamp, the acid lost 0·129 more, making the whole loss amount to 35 per cent. Ten grammes of crystallized boracic acid were mixed with 40 grammes of pure oxide of lead and a quantity of water sufficient to dilute the mixture. It was digested till the boracic acid had combined with the oxide of lead. It was then evaporated to dryness, and exposed to a red heat. The calcined mass weighed 45·6 grammes ; therefore, 4·4 grammes of water had separated from the crystallized acid by its union with the oxide of lead. This is precisely twice as much as the acid had lost by the heat of a sand-bath. The experiment appears then to prove that crystallized boracic acid contains two portions of water ; one of which serves as a salifiable base ; while the other is the water of crystallization. This last portion is disengaged by a low heat, while the other requires a higher temperature, or the addition of a base, to be separated entirely from the acid. The preceding experiments lead to the supposition that the water which serves as a base to the acid, separates at two different periods; one half separates first, and the other remains united to the acid, in the the form, to speak so, of a subborate of water.

Ten grammes of crystallized borate of ammonia were mixed in a retort with 40 grammes of pure lime. The retort was adapted to a small tubulated receiver filled with caustic potash, and furnished with a glass tube likewise filled with caustic potash, through which the ammoniacal gas was to issue. The receiver and tube with the potash were exactly weighed before the experiment. The retort was then slowly heated, till at a heat nearly red, no more ammoniacal gas was disengaged. The apparatus was then allowed to cool, the receiver was removed, and the ammoniacal gas driven out of it by means of a current of air which passed over muriate of lime before entering into the receiver. The receiver and tube had acquired 3·173 grammes of water, and in the retort, 3·795 grammes of boracic acid remained united with the lime. The loss, amounting to 3·032

grammes, must be ascribed to the ammoniacal gas. Thus borate of ammonia is composed of

Boracic acid	37·95	100·000
Ammonia	30·32	79·895
Water	31·73	
	100·00	

This quantity of water contains 28 of oxygen; and the ammonia, on the hypothesis that it contains a quantity of oxygen proportional to the quantity of acid which it saturates, compared with the other saline bases, contains 14·07 of oxygen, which, multiplied by 2, gives 28·14; so that in this salt, as well as in the sulphate and oxalate of ammonia, the water of combination contains twice as much oxygen as the base. The 79·895 of ammonia, with which 100 parts of boracic acid are saturated, contain 37·085 of oxygen; and if boracic acid contain twice as much, it is composed of 74·17 per cent. of oxygen and 25·83 of boron, which comes very near the determination of Davy. The result furnished by the analysis of borate of ammonia is still further confirmed by the late analyses of the biborate of magnesia (*boracite* of mineralogists) in which 100 of boracic acid saturate a quantity of base which contains 18·54 of oxygen; that is, half the number found above.

I was desirous to confirm this experiment by the analysis of the borates of barytes and lead; but these salts always gave variable results after a lixiviation continued for a longer or shorter time, by means of which these borates are decomposed, the water carrying off a portion which is at a different degree of saturation from that which remains. Besides, the borate of barytes precipitated by borax contains always a greater proportion of acid than the borate of soda; so that this last precipitate is a mixture of borate and biborate of barytes.

I could have wished to verify these experiments on the composition of boracic acid by synthesis; but having no boron in my possession, I was obliged to give up the project.

Experiments on the probable Composition of Fluoric Acid, calculated from its Capacity of Saturation.

Different chemists have endeavoured to determine the capacity of saturation of fluoric acid by analysing fluate of lime. I may mention Wenzel, Richter, Klaproth, Dalton, Thomson, and Davy; but their experiments have given results so variable that Klaproth, for example, found the capacity of saturation of this acid a third greater than Dalton. Sir Humphry Davy obtained from 100 of the fluate of lime of Derbyshire, by digesting it eight times successively with sulphuric acid, 175·2 of gypsum. In an analysis of a fluate of lime from the iron mine of Rosberg, 100 parts of the fluate gave me 173 of sulphate of lime. I was

of opinion, however, that less confidence should be put in these experiments than in others made by Dr. John Davy on fluosilicic acid and its combinations with ammonia. These experiments. give the capacity of saturation of fluoric acid still greater than the analysis of fluate of lime by Sir H. Davy. I employed it not only to determine the composition of some fluates, but likewise to ascertain the composition of silica. However, the results of Dr. Davy's experiments are inaccurate, as I shall show hereafter, when treating of the composition of silica; but the error resulting from it in the calculations cannot be immediately discovered, because upon the fluates, it makes a difference of only 2 per cent. and with respect to the composition of silica, another inaccuracy in a contrary way corrects the results of the calculation. However, as it was necessary for me to know with much precision the composition of silica, and as I thought that the analysis of fluosilicic acid gas might be serviceable for that object, I wished to begin by studying the capacity of saturation of fluoric acid. To avoid the objection of the presence of silica in the fluate of lime, 1 chose for my first experiments fluate of silver.

To prepare it, I distilled, in a small platinum apparatus, fluate of lime with sulphuric acid. The acid was received by water in a platinum cup. I then added carbonate of silver to the liquid till it was saturated. There fell a slight greyish precipitate which I considered as fluosilicate of silver. The neutral liquid was filtered through paper in a platinum funnel, as I had found that it corrodes glass, and deposits in that case the greyish precipitate of which 1 have spoken. The liquid was then evaporated and the mass heated to redness. It melted, and continued always to give out fluoric acid gas and oxygen gas, while metallic silver was disengaged. This phenomenon continued as long as the fluate remained exposed to the fire; and I have reason to believe that it is not owing to the presence of water.

4·936 grammes of fused fluate of silver left, when dissolved in water, 0·185 gramme of metallic silver. The liquid precipitated by sal-ammoniac produced a quantity of muriate of silver, which weighed, after being fused, 5·349 grammes; that is to say, that 100 of fluate of silver had given 112·587 of muriate of silver.

9·922 grammes of fluate of silver fused and redissolved in water left 0·376 gramme of metallic silver, and furnished 10·7465 grammes of muriate of silver. These two numbers are to each other as 100 : 112·57.

These two experiments agree as nearly as possible. According to them, 100 of fluoric acid combine with 1021·8 of oxide of silver, of which the oxygen is 70·4, which in this case ought to be the number which represents the capacity of saturation of fluoric acid.

Fluate of Barytes.—A portion of fluate of soda slightly acid was evaporated to dryness, and slightly heated, without, how-

ever being made red-hot; after which it was redissolved in water, which left a slight trace of silica undissolved. This solution was mixed with muriate of barytes. The precipitate separated on a filter and washed, appeared to dissolve in water, though in a very small proportion; so that the water employed to wash it was always precipitated by sulphuric acid. After having passed through it a quantity of water sufficient in other cases to have washed such a portion of matter, I dissolved a small quantity of the precipitate in nitric acid. On pouring in a small quantity of nitrate of silver, muriate of silver was precipitated in abundance. It appears then that the precipitate formed by the addition of fluate of soda to the muriate of barytes is a double salt composed of the two acids united to a single base. I did not examine it more closely; but satisfied myself with ascertaining that it could not be employed for the object which I had in view.

To obtain pure fluate barytes, I employed a solution of nitrate of barytes, which I poured into a solution of fluate of soda, taking care that the whole quantity of fluoric acid was not precipitated. The precipitate being washed, dried, and exposed to the fire, gave out fluoric acid without any trace of the smell of nitrous acid. It was necessary to expose the salt to a red heat repeatedly before it ceased to lose weight. This circumstance would have deserved a particular examination, if the number of experiments necessary for the object which I had in view had not prevented me from touching upon any thing that could give a different direction to my labours.

Six grammes of this fluate of barytes, long exposed to heat, were decomposed by sulphuric acid, and produced 7·968 grammes of fluate of barytes. According to this experiment, 100 fluoric acid were united with 697·7 of barytes, the oxygen of which is 71.

Fluate of Lime.—I made choice of a fine specimen of Derbyshire fluate of lime, forming a large, colourless, transparent crystal, which I considered as sufficiently pure to deserve to be analysed. I reduced it to powder upon a slab of flint, and I levigated the powder with great care, so as to render it perfectly impalpable.

Ten grammes of this powder, heated some degrees above 212°, were exposed in a platinum crucible to an incipient red heat without any loss of weight. I then exposed it for some time to a red heat, but still it lost no weight. It follows from this, that when the water, mechanically adhering to the powder, has evaporated the fluate of lime, undergoes no further alteration in the fire. I now mixed the powder with pure sulphuric acid, employing a platinum spoon, whose weight had been determined along with that of the crucible. The first effect of the action of the sulphuric acid was, that the mass augmented considerably in

volume, and became gelatinous and semi-transparent, as by a solution in the acid without decomposition. No effervescence took place. At a higher temperature, the mass began to exhale vapours of fluoric acid in abundance, became white, and lost its transparency in proportion as the fluoric acid was disengaged. The sulphate of lime was then exposed to a red heat to drive off the excess of sulphuric acid. It was white as snow, and weighed 17·363 grammes. I then added a new quantity of sulphuric acid which I allowed to digest for some hours with the sulphate, and the excess of acid was then entirely dissipated by the fire. The weight of the sulphate of lime was not in the least increased.

Ten grammes of fluate of lime treated in the same way produced in a second experiment 17·368 of sulphate of lime, dried in a red heat. According to these experiments, fluate of lime is composed of

Fluoric acid .... 27·863 ...... 100·0
Lime .... 72·137 ...... 258·9
                    ————
                    100·000

The 258·9 of lime contain 72·7815 of oxygen, which consequently ought to represent the real capacity of saturation of fluoric acid.

In examining these experiments, we find that both the inaccuracies of the method of operating, and the foreign substances in the fluate of lime, ought all to concur to render the quantity of sulphate of lime obtained too small; and that no other circumstance, except the impurity of the sulphuric acid, could increase the quantity of that salt. To verify this point, I evaporated in a platinum crucible 150 grammes of the sulphuric acid employed, but the weight of the crucible was not altered. It is obvious then that the analysis of fluate of lime has given a more exact result than that of the other fluates analysed above. The circumstance that the fluate of lime becomes transparent, and appears to combine without decomposition with cold sulphuric acid, ought to guarantee the absence of silica, as the slightest trace of that earth determines the immediate disengagement of silico-fluoric gas, which is manifested by a greater or smaller effervescence, which sometimes occasions the mass to run over the vessel in which it is contained. It is probable that the reason why the artificial fluates have given the ratio of the acid to the base greater than the fluate of lime is, that they contained a small quantity of silica which could not be entirely separated from them, and which, in the analytical experiments, accompanied the fluoric acid.

When we seek to determine from the capacity of saturation of this acid what is the quantity of oxygen which it should contain, considering it as an acid with oxygen, whose combustible

radical has never yet been separated, it is clear that this acid can only contain a quantity of oxygen equal to that contained in the base. In that case, fluoric acid will be composed of

$$
\begin{aligned}
&\text{Fluorine} \ldots\ldots\ldots\ldots\ 27\text{·}22 \ \ldots\ldots\ 100\text{·}00 \\
&\text{Oxygen} \ldots\ldots\ldots\ldots\ 72\text{·}78 \ \ldots\ldots\ 262\text{·}84
\end{aligned}
$$

(To be continued.)

Article VII.

A Chemical Analysis of the Cælestine (Sulphate of Strontian) found at Nörten, not far from Hanover.[*] By M. Gruner Ober-Berg, Commissair in Hanover.

Though the neighbourhood of Hanover is rather poor in minerals, yet there has lately been observed cælestine both in crystals and weathered in a quarry opened for procuring materials for mending the roads. I have analysed several of these specimens, and have obtained results indicating that they constitute a new variety of sulphate of strontian.

The place where this mineral is found is at a village called Nörten, about two hours' journey from Hanover, where the quarry containing the sulphate of strontian is opened at the foot of a mountain. The rock in which it occurs is a large grained floetz limestone, which contains here and there petrified encrini and nummuli, and thin veins of galena. The sulphate of strontian hitherto observed passes through the limestone in three perfectly parallel beds (Trummen), distant from each other from 24 to 30 inches, and inclined at an angle of from 60° to 70°. The breadth of these beds does not exceed two inches. In the first of these beds, the mineral occurs in a crystallized state; while in the second and third it has a weathered aspect; and in the third bed this has gone so far that the mineral has an earthy aspect. The first bed is highly impregnated with *clay iron hydrate*, by which the strontian contained in it is coloured brownish-red. Besides, in these three beds, sulphate of strontian is found likewise scattered through the limestone in fine feathery crystals.

The whole of the first bed is filled with the strontian, which has a foliated or radiated texture, and a silky lustre. Its colour is usually milk-white, but here and there it passes into blue. In the second bed, the strontian still retains its form; but it has quite lost its lustre and its colour. The change is still more conspicuous in the third bed where the strontian has lost its form likewise.

[*] Translated from Gilbert's Annalen, vol. ix. p. 72.

The specific gravity of the crystallized mineral I found to be 3·5906 at the temperature of 72¼°.

The chemical analysis of this mineral was conducted by me in the following manner:

(A.) Two hundred grains of the mineral were reduced to a fine powder, exposed for half an hour to a red heat, and then weighed while still hot. No alteration in the weight could be perceived.

(B.) One hundred and fifty grains of the mineral were reduced to a fine powder in an agate mortar, mixed with thrice their weight of carbonate of potash, and heated for two hours so strongly in a silver crucible that the mass had melted. The matter when cold was softened in distilled water, and then washed till the water employed for that purpose ceased to be acted on by reagents.

(C.) The alkaline solution obtained in this way was supersaturated with muriatic acid, evaporated to dryness, the dry mass was redissolved in distilled water, and the excess of acid saturated with potash without any muddiness or precipitate making its appearance.

(D.) The earthy residuum was now treated with muriatic acid, in which it dissolves completely, and the solution was accompanied by a strong effervescence. Into this muriatic solution was added an excess of ammonia, whereby a small quantity of a light brownish-red precipitate was thrown down, which, when dried and heated to redness for half an hour in a platinum crucible, weighed 0·32 gr. I dissolved it in a few drops of muriatic acid, diluted the solution with water, and mixed it with prussiate of potash, by which the iron was thrown down in the state of a prussiate. The liquid thus freed from iron being mixed with ammonia deposited some alumina, but the quantity of it was so small that it could not be weighed. This 0·32 gr. constitutes the ferruginous alumina with which the strontian is mixed in the mineral.

(E.) The liquid thus supersaturated with ammonia was evaporated to dryness in a porcelain vessel, the dry residue was heated to redness in a platinum crucible till all the sal-ammoniac was driven off. It was then reduced to a fine powder, and digested in 16 times its weight of alcohol. The alcohol was raised to the boiling temperature, and decanted, while boiling hot, from the undissolved residuum.

The alcoholic solution on cooling let fall fine needle-form crystals. These crystals continued to be deposited during the evaporation to the very last drop. The crystals thus obtained were divided into two equal halves. The one was employed in experiments to learn the nature of the salt. It exhibited the properties of muriate of strontian. The remaining half of the crystals were dissolved in water, and the solution poured into the alkaline ley (C) saturated with muriatic acid. A white heavy powder fell, which was sulphate of strontian. This regenerated sulphate of strontian weighed, after being heated to redness in a

platinum crucible, 54·75 gr. Hence it follows that the whole sulphate of strontian contained in the mineral amounts to 109·5 gr.

(F.) The portion of salt which did not dissolve in alcohol was dissolved in water, and the solution evaporated till the salt was obtained in crystals. These crystals were tables, and possessed the characters of muriate of barytes. Being dissolved in water, and mixed with the liquid from which the sulphate of strontian had been separated by filtration, a precipitate of sulphate [of barytes fell down, which, when washed, dried, and heated to redness, weighed 39·25 gr.

From the preceding experiments, it appears that the 150 gr. of the mineral were composed of

Ferruginous alumina (D)	0·32
Sulphate of strontian (E)	109·50
Sulphate of barytes (F).	39·25
	149·07
Loss.	0·93
	150·00

Consequently the constituents in 100 parts of the mineral are as follows :

Ferruginous alumina	0·213
Sulphate of strontian...............	73·000
Sulphate of barytes.	26·166
	99·379
Loss	0·621
	100·000*

This cælestine is remarkable for the great proportion of sulphate of barytes which it contains. I have never before heard of any cælestine containing more than two or three per cent. of this salt. This circumstance induced me to repeat the analysis again ; but I obtained almost exactly the same result. We must, therefore, as I observed before, consider this mineral as constituting a new variety of cælestine hitherto unknown.

These remarks apply likewise to the weathered cælestine in the third bed. I analyzed it in the same way as the crystallized cælestine, and obtained the following result :

Alumina.	1·00
Sulphate of strontian.	24·00
Sulphate of barytes	74·66
	99·66
Loss	0·34
	100·00

* This is almost exactly seven atoms of sulphate of strontian to two atoms of sulphate of barytes. Hence it is probable that the mineral is a chemical compound.—T.

Thus the sulphate of barytes in this mineral exists in the same proportion as the sulphate of strontian in the crystallized specimens. Is this owing to the washing out of the sulphate of strontian by the action of water, which has probably produced the alteration observable in the state of this bed? This weathered mineral may, from the great proportion of barytes which it contains, be considered as a new variety of sulphate of barytes.

In the collection of minerals exposed to sale by Mr. Geissler, in Göttingen, amateurs may obtain fine specimens both of the foliated cælestine, of the feathery varieties, and of the weathered kind, as that gentleman was the first person who examined the quarry; and he supplied himself with abundance of specimens of the strontian, which was just then making its appearance.

ARTICLE VIII.

ANALYSES OF BOOKS.

Description of the Process of manufacturing Coal Gas for the Lighting of Streets, Houses, and Public Buildings, with Elevations, Sections, and Plans, of the most improved Sorts of Apparatus now employed at the Gas Works in London, and the principal provincial Towns of Great Britain; accompanied with comparative Estimates, exhibiting the most economical Mode of procuring this Species of Light. By Fred. Accum, Operative Chemist, &c.   London, 1819.

THIS work is intended to supersede a former treatise written by the author upon the same subject, by conveying to the public a particular detail of the newest improvements introduced into the manufacture of coal gas, and a full account of every thing connected with the subject likely to interest the public.   Mr. Accum has had considerable experience in the erection of gas works, and he has availed himself of the experience acquired in the gas works established in London and other places.   The consequence is, that he has been enabled to communicate a great deal of information in the present work.   I cannot do better than recommend it to those who are interested in such subjects.   It will fully repay the trouble of a perusal.

He has divided the work into 16 parts.   The first part is employed in pointing out the advantages of this new mode of procuring light.   It is, perhaps, not the least interesting chapter of the book; but as it conveys no new scientific information, I shall pass it over without any particular analysis.

The second part consisting of only a few pages gives the reader some idea of the mode of preparing *coal gas*.   This gas is usually considered as *carburetted hydrogen gas*.   I believe it always

contains a portion of that gas; but I have never met with any coal gas consisting of pure carburetted hydrogen. It has always proved, in the cases where I had an opportunity of examining it, a mixture of carburetted hydrogen, carbonic oxide, and hydrogen gas, the proportions of which vary according to the nature of the coal and of the process. When the heat is applied suddenly, and when it amounts to a good red heat, the proportion of carburetted hydrogen is greatest, and when the heat is low, the proportion of pure hydrogen is greatest. Olefiant gas and sulphuretted hydrogen are probably likewise present; though in small and variable quantity. There is another circumstance connected with this gas, which has not hitherto been noticed; but which must have some influence upon the light which it yields. Coal gas has always the very same smell as the oil or naphtha which coal yields when distilled; therefore, it obviously contains a certain portion of naphtha mixed with it in the state of vapour. When naphtha is put in contact with a quantity of common air, or indeed of any gas whatever, a portion of it mixes with the gas in the state of vapour, and communicates to it the peculiar smell by which it is distinguished. Gas thus contaminated with the vapour of naphtha is not easily purified again. It may be allowed to remain in contact with water, or even passed through water without losing any of the naphtha vapour. The quantity of this vapour contained in coal gas depends upon the temperature of the naphtha and gas when placed in contact. At the temperature of 55° the bulk of air, when placed in contact with naphtha, is increased 3 per cent. I find that the specific gravity of vapour of naphtha is 2·26, that of common air being 1·00. From this it will not be difficult to determine the quantity of naphtha with which coal gas is usually contaminated. One volume of vapour of naphtha for complete combustion requires rather more than 2·4 volumes; but not quite so much as 2·5 volumes of oxygen gas.

As carburetted hydrogen gas, carbonic oxide, hydrogen, and olefiant gases, are all destitute of smell, and as coal gas has always a strong smell of naphtha from which it cannot be, or at least has never yet been deprived, I conceive, that the presence of the vapour of naphtha in it will not admit of a doubt.

In the third part our author gives a classification of pit-coal as far as the production of coal gas is concerned. I need scarcely remark, that our author's division is very imperfect indeed; and that, as he gives no description of the varieties which he names, the division, imperfect as it is, is of little or no use. For an idea of the composition, which will enable the reader to form some accurate ideas on the subject, I refer to a paper of mine on the coal found near Glasgow, published in the *Annals of Philosophy*, xiv. 81. Mr. Accum divides coal into three classes:

1. Those kinds that contain much bitumen. The following table exhibits the maximum quantity of gas obtainable from coals belonging to this class:

One chaldron of coal produces   Cubic feet of gas.

Scotch cannel coal.	19,890
Lancashire Wiggan coal	19,608
Yorkshire Cannel coal (Wakefield).	18,860
Staffordshire coal,*	
First variety.	9,748
Second variety	10,223
Third variety	10,866
Fourth variety.	9,796
Gloucestershire coal,†	
First variety (Forest of Dean, High Delph)	16,584
Second variety (Low Delph)	12,852
Third variety (Middle Delph)	12,096
Newcastle coal,	
First variety (Hartley)	16,120
Second variety (Cowper's High Main)	15,876
Third variety (Tanfield Moor)	16,920
Fourth variety (Pontops)	15,112

2. The second class consists of coals containing a smaller quantity of bitumen, and more charcoal than the preceding. The following table exhibits the maximum quantity of gas obtainable from coals belonging to this class:

One chaldron of coal produces   Cubic feet of gas.

Newcastle coal,	
First variety (Russel's Wall's End)	16,876
Second variety (Bewick and Craister's Wall's End)	16,897
Third variety (Heaton Main).	15,876
Fourth variety (Killingsworth Main).	15,312
Fifth variety (Benton Main)	14,812
Sixth variety (Brown's Wall's End).	13,600
Seventh variety (Mannor Main)	12,548
Eighth variety (Bleyth).	12,096
Ninth variety (Burdon Main).	13,608
Tenth variety (Wear's Wall's End)	14,112
Eleventh variety (Eden Main)	9,600
Twelfth variety (Primrose Main)	8,348

3. The third class consists of coals that yield little or no bitumen when distilled. The following table exhibits the maximum quantity of gas obtainable from this class of coal.

* They require a much higher temperature than is necessary for the decomposition of Newcastle coal.

† Most varieties afford a porous and very friable coke.

One chaldron of coal produces   Cubic feet of gas.

Welch coal,
    First variety, from Tramsaren, near Kidwelly 2,116
    Second variety, from the yard vein at the same place... 1,656
    Third variety from Blenew, near Llandillo. 1,416
    Fourth variety from Rhos, near Ponty Barren........ 1,272
    Fifth variety from the Vale of Gwendrath 1,292
    Sixth variety from ditto. 1,486

In Part IV. the author gives us an account of the retorts employed for manufacturing coal gas. They are made of cast-iron, and a cylindrical shape has been ultimately preferred. They are 6½ feet long, and 1 foot in diameter. Five of these are now arranged in a kind of oven, which are raised to the requisite temperature by means of three fires below, and so contrived that the flame plays equally round the retorts. For the contrivances for conveying away the gas and separating the tar and the ammoniacal liquor, I must refer to the work itself. The quantity of coal necessary to be burned as fuel in order to decompose 100 lbs. of coal, varies from 20 lbs. to 25 lbs.

The greatest quantity of gas is disengaged at first, and the quantity of product diminishes continually during the process. But the author proves in Part V. by satisfactory experiments made on a large scale, and repeated for a sufficient length of time, that it is more economical to continue the application of the heat to the retorts for eight hours than to stop after an interval of six hours.

In Part VII. the author shows that in London it is more economical to decompose the coal by a strong red heat, though the retorts are sooner destroyed, than to employ a low heat, which yields less gas, but allows the retorts to last longer. The retorts ought to be made of light-grey cast-iron, not of the softest kind, which is unable to resist a continued heat. They must be always kept red-hot as long as they last. If they be allowed to cool, they waste much sooner. In London, 1000 cubic feet of gas can be made for seven shillings. Cannel coal it appears yields its gas at a lower temperature than any other, and is, therefore, best adapted for making coal gas.

One of the most curious parts of the coal-gas machinery is what is called the horizontal rotary retort invented by Mr. Clegg, and gradually brought to perfection. Our author employs the seventh part of his work in explaining this curious apparatus. We cannot do better than quote this article (abridging it a little) as a specimen of the work, which is likely to interest the reader.

Horizontal Rotary Retorts, lately brought into Use for manufacturing Coal Gas.

The many disadvantages attendant on the plan of decomposing coal in masses from five to ten inches in thickness, as already sufficiently exposed in the preceding parts, had naturally the effect of developing a principle of manufacturing coal gas, which practice has now fully established, namely, that to decompose coal, in

thin layers from two to four inches in thickness, is to obtain the greatest quantity of gas from a given quantity of coal at the least expense.

Mr. Clegg was the first person who pointed out to the public the advantages that must accrue from this mode of operating, and to him we are indebted for the construction of an apparatus, the great ingenuity and superiority of which entitles what is called the horizontal rotary retort, to all the merit and praise that belongs to the character of an original invention.

The numerous and great advantages of this distillatory apparatus, the rapidly increasing adoption of it, * and the almost certain prospect which exists of their ultimately superseding all former methods of decomposing coal, make it proper that I should lay before the reader as full an account as my limits will permit, of the construction and operation of this retort, and the mode of applying it; and this becomes the more necessary on account of the many important improvements which the apparatus has undergone since its first adoption, † and of which no description has yet been laid before the public.

The following account will render the construction of this retort sufficiently obvious:

Description of the Horizontal Rotary Retorts at the Royal Mint. ‡

The horizontal rotary retorts at the Royal Mint are hollow cylinders, eight feet six inches in diameter, and 15 inches high, arched a little at the top. They are made of wrought-iron plates, half an inch thick, rivetted together in the manner of a steam-engine boiler. A, A, A, fig. 1, Plate CIII. exhibits a perpendicular section of the rotary retort. In fig. 2, the retort is seen fixed in the brick-work; a, fig. 2, shows the mouth of the retort, through which the coals are introduced, and from whence the coke is withdrawn. It is also shown in perspective at B, B, B, fig. 6. The mouth is closed with a cast-iron door fitted on air-tight by grinding.

The door is connected at its upper and lower extremities, with a frame and adjusting rod, see B, B, fig. 2, by means of which it may readily be slided down below the mouth of the retort, when the coals are to be introduced, or coke is to be withdrawn. To the upper extremity of the rod B, fig. 2, is fixed a lever, loaded with a counterpoise weight C, to balance the door, and to render the opening and closing of it easy and expeditious.

The mouth-piece and its door is three feet long, and nine inches wide; it projects nine inches beyond the brick-work or furnace in which the retort is fixed, as may be seen at a, fig. 2.

The fire-place, which is on the opposite side to that of the mouth of the retort, heats only one-third part of the whole capacity of the retort to that degree which is proper for the complete and rapid decomposition of the coal; while the remaining parts, which are not over the fire-place, and to which the fire flues do not extend, are kept at a lower temperature.

The flues are directed under about one-third of the area of the bottom of the retort, and after having passed over one-third part of the area of the top of the retort, they pass into the chimney. Fig. 5, exhibits the direction of the flues; A, A, the flues and the fire-place. The whole retort is guarded from the contact of the fire, which would soon destroy it, by fire-bricks; it notwithstanding speedily receives the full effect of the heat, and retains its temperature when once heated for a long time. Fig. 2, exhibits one of the retorts fixed in its furnace. A perspective view of three retorts may be seen in fig. 6.

Through the centre of the retort passes perpendicularly an iron shaft, D, as shown in the section of the retort, fig. 1, and also in fig. 2. The lower extremity of the shaft revolves upon the bottom of the retort, in a cup-shaped cavity, while its upper extremity passes through the roof of the retort, where the latter is made air-tight by means of a pipe, E, fig. 2, and E, fig. 1, closed at the top and surrounding the shaft; and hence the shaft must always preserve its centre.

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* Retorts of this description have been lately adopted in the gas works at Bristol, Birmingham, Chester, Kidderminster, and at many other provincial gas establishments.

† An account of the original construction of the rotary retort may be seen in the Repository of Arts, No. CLXXVI. 1816, p. 1; and also in the Journal of Science, vol. ii. p. 133.

‡ The retorts lately erected at the gas works at Birmingham, Chester, Bristol, &c. are similar to those at the Mint.
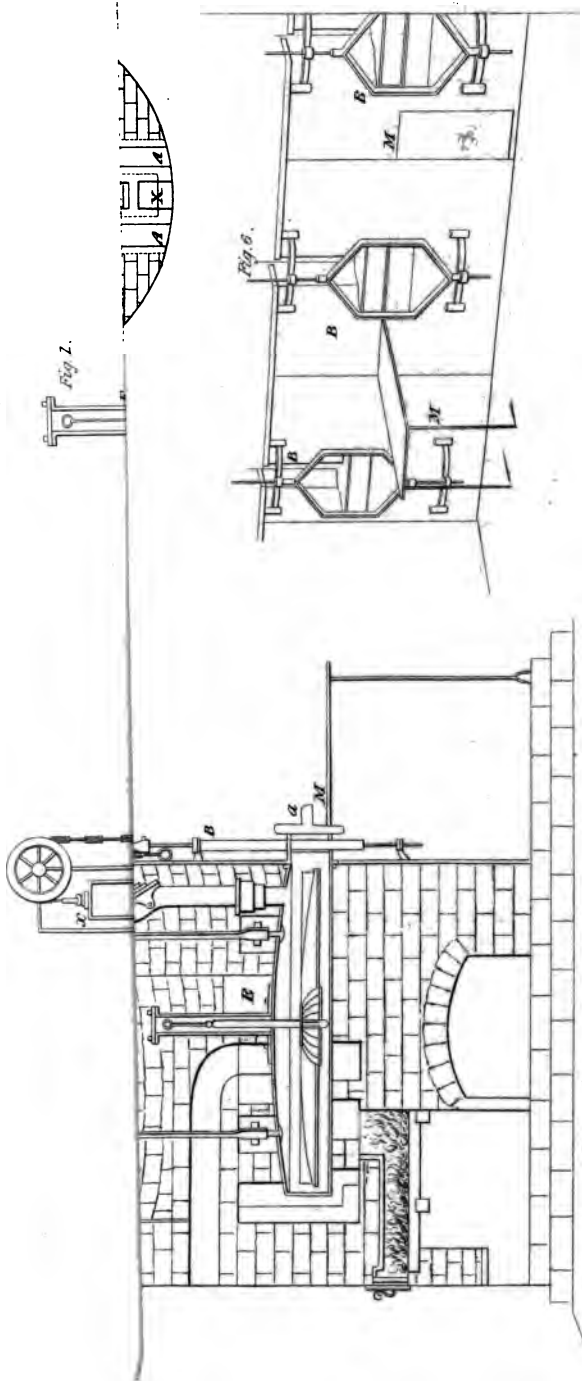
Fig. 7.

Fig. 6.
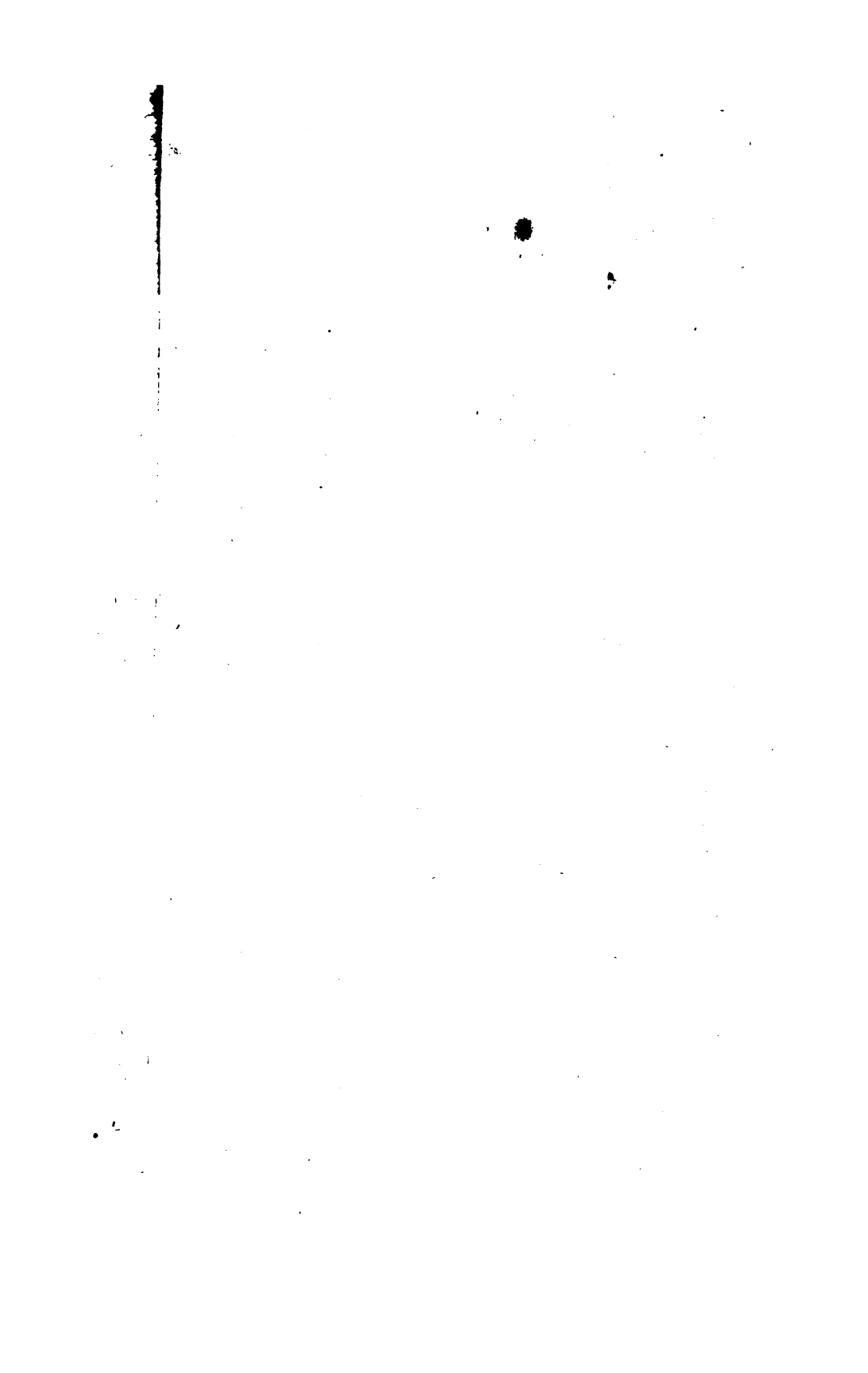
To the lower extremity of the shaft is keyed a box, or centre piece (technically called a rose centre), F, fig. 1. It is also seen in the perpendicular section of the retort, fig. 2. From this shaft radiate 12 wrought-iron arms, G, G, fig. 1,* fixed in sockets made in the box. These arms are elevated three inches above the bottom of the retort, and extend to nearly within its whole inner circumference. They are wedge-shaped, and their greatest diameter is at right angles to the base of the retort; so that the weight of the arms rests on the axis. They are intersected by two concentric rings, as will be seen on inspecting fig. 3, which exhibits the plan of the retort, together with the iron arms, G, G, and concentric rings. The centre of fig. 3, shows also the plan of the rose centre, F, fig. 1, into which the arms are keyed.

Between the arms are placed 12 shallow iron trays or boxes, destined to contain the coal from which the gas is to be obtained. They are formed to the segment of a circle; hence the whole series of them, when arranged in the retort, exhibits a shallow circular tray, which, when motion is given to the shaft, may be made to revolve within the retort.

Fig. 4, exhibits one of the shallow trays, or coal boxes, in perspective.

It will be obvious that by the motion of the shaft, any number of the trays, or coal-boxes, can readily be brought from the coldest into the hottest, and from the hottest into the coldest part of the retort.

H, fig. 2, and a, fig. 1, is a perpendicular pipe situated at the margin of the retort, close behind the mouth-piece, and consequently in the coldest part of the retort. It serves to carry off the distillatory products evolved from the coal, and causes part of the vapourous tar, which becomes condensed in it, to trickle back again upon the coal in the retort, in order to become converted into gas, when the coal on which it falls becomes situated over the fire-place.

This pipe is furnished at its upper extremity with a *hydraulic valve*, I, fig. 2. It consists simply of an inverted cup, X, applied over the upper open extremity of the perpendicular pipe, H, and submersed into a cup formed of a portion of larger pipe, surrounding the pipe, H, containing tar. The smaller, or inner cup, X, is represented in the design raised out of the liquid contained in the outer cup, J, to show an aperture, Y, made in the smaller or inner cup; the use of which will be mentioned hereafter. The inverted cup, X, is furnished with a chain, one extremity of which is fastened to the upper extremity of the cup, the other passes over a small wheel, and descends through the roof of the building, as shown in the design.

K, fig. 2, is a branch pipe proceeding laterally from the perpendicular pipe, H; it communicates with the hydraulic box, L. N, is a pipe which proceeds from the hydraulic box, L; it serves to carry away the gaseous and liquid products to their places of destination.

M, fig. 2, or fig. 6, is an iron flap table, placed level with the bottom of the mouth of the retort. It is convenient to hold several coal trays ready charged with coal in a state fit to be introduced into the retort.

The fire-place, flues, and ash-pit, of the furnace, in which the retort is fixed, are sufficiently obvious, by mere inspection of fig. 2. The front elevation of the retort is seen in fig. 6, which exhibits three horizontal retorts; two of which have the door of the mouth-piece slided down, and one with the door in its place, or shut. The circular ring seen in this design, at the top of each retort, which rests on iron-bearing bars, the extremities of which are let into the end walls of the furnace, serves to support the roof of the retort by means of bolts proceeding from the inner side of the roof. This arrangement is likewise shown in the section, fig. 2. At the bended part of the perpendicular pipe, H, fig. 2, is seen a bonnet, or cover, which closes an opening made into the pipe, H, through which, by means of an iron rod, the lower extremity of the pipe, H, may, from time to time, be examined, to guard against an incrustation of decomposed tar or carbonaceous matter that might happen to accumulate in that part of the pipe. The upper part of the pipe, H, above the bonnet at the bended part, requires no examination.

b, fig. 1, and b, fig. 3, is the flanch of the retort; c, fig. 1, the flanch of the mouth-piece; d, the cutter, or wedge, which draws the mouth-piece close; e, the cross bar, against which the cutter, d, bears, to render the mouth-piece air-tight;

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* In the horizontal rotary retorts at the Chester, Birmingham, and Bristol gas works, which are 12 feet 6 inches in diameter, there are 15 arms. At some gas works, the arms are made of cast-iron.

f, fig. 1, one of the eye-bolts, or arms, which support the cross bar, *e*; it is also seen at *e* in the plan of the retort, fig. 3. In this figure, *b* is the flaunch of the retort, and *c*, the door.

These few particulars will be sufficient to enable the reader to understand the construction of the retort; its action is as follows:

Action and Management of the Horizontal Rotary Retort.

When the retort is heated to the proper temperature for the decomposition of the coal, the door is slided down, and the coal-boxes charged with small coal are slided into the retort from the table, M, fig. 2, one by one, so that each box rests firmly upon the concentric rings placed between the arms of the retort; the door is then slided up again into its place, and rendered air-tight by means of wedges.

When the whole circle, fig. 3, is thus filled with coal-boxes (the coal should be spread in the boxes, in layers two or three inches in depth), it is obvious that of all the 12 boxes, four only can be situated directly over the fire-place, while the remaining eight are placed right and left towards the door of the retort. The coal in the former boxes receives the full effect of the heat (see the plan of the fire flues of the retort, fig. 5), while the remaining eight boxes, to which the fire does not extend, are less heated. The coal in the four boxes which are in the hottest part of the retort becomes rapidly decomposed, while the coal in all the other boxes is gradually heated, and consequently deprived only of moisture, previous to being subjected to the greatest heat. The box, which is situated under the condensing pipe, H, fig. 2, near the entry door, receives the condensed tar which trickles down the pipe, H.

Now let us suppose that the coal in the four boxes over the fire place is fully decomposed, which will be the case if $32\frac{1}{2}$ lbs. of coal are in each box, in two hours, the workman then turns the shaft, E, fig. 2, one-third part of the circumference of a circle, by pulling towards him by means of an iron hook the nearest iron arm that may happen to be opposite to the door; this moves those boxes which at the commencement of the operation were over the fire-place, towards the coldest part of the retort; namely, towards the door which is opposite to the fire-place, and a second series, or four of the adjacent boxes, are brought in turn into the hottest part of the retort, or over the fire-place, from whence the preceding boxes were removed.

When the coal in the second series of boxes has been two hours in the hottest part of the retort, its decomposition will be completed; the workman therefore turns the shaft again one-third part of a circle, and a third series advances in their place, while at the same time the first series becomes situated opposite the entry door of the retort, from whence they may be withdrawn and exchanged for an extra set of trays, ready charged with coal, and placed on the iron table for that purpose.

In this manner the operation proceeds. One-third part of the whole charge of coal within the retort is always in the act of becoming decomposed; another third part is gradually heated, and totally deprived of moisture, previous to its being exposed to the temperature necessary for its decomposition; and the remaining third part placed in the coldest part of the retort receives that portion of tar, which escapes decomposition, and trickles down the perpendicular pipe, in order to be decomposed, when the coal upon which it falls becomes situated over the fire-place. Hence the quantity of tar obtained from one chaldron of Newcastle coal, when decomposed by means of an horizontal rotary retort, seldom amounts to more than 60 or 70 lbs. whereas the same quantity of coal, when decomposed by means of cylindrical or parallelopipedal retorts, yields never less than from 150 to 180 lbs. An horizontal rotary retort, 12 feet 6 inches in diameter, and 15 inches high, furnishes, in the ordinary way of working, every 24 hours, 15,000 cubic feet of gas, when five trays of the retort are charged with three bushels of Newcastle coal. The weight of the retort is three tons; its capacity 150 cubic feet.

The hydraulic valve, above described, serves merely to restore the equilibrium between the gas within the retort and the atmospheric air without, previous to the opening of the door of the mouth of the retort. To effect this, the workman raises the cup, X, by means of the chain; so that the small hole, Y, in the cup, X, becomes raised out of the tar in the cup, I, and he closes it again when the retort is charged: this operation requires two minutes. We have stated already, that the door of the retort is ground air-tight; and hence it requires no luting.

Advantages of the Method of manufacturing Coal Gas by Means of Horizontal Rotary Retorts.

The advantages of the mode of manufacturing coal gas by means of horizontal rotary retorts, consist in a saving of fuel, time, labour, and machinery, a gain in the quantity of gas, and increase in the quantity of coke.

Saving of Fuel.—The mass of coal subjected to decomposition being reduced from the dimensions required in the old plan (by means of cylindrical retorts) to the narrowest available limits, there being no outward crust of coke to be kept red-hot for hours to no purpose, while the decomposition of the interior mass of coal is going on ; the coke itself being as soon as formed removed from the source of heat, and applied, while cooling, to warm up a fresh supply of coal next in order of becoming decomposed, instead of being discharged in a red-hot state, into the open air, as requires to be done in the practice before detailed—the whole fuel in short being necessarily and beneficially expended—the saving of coal employed as fuel in this respect, is exactly the gaining of all that is lost on the plan of employing cylindrical or any of the retorts before described. Hence one chaldron of coal is decomposed at the gas establishments where horizontal rotary retorts are in action by means of 20 per cent. of fuel, and at some establishments an expert stoker will work the retorts with 15 per cent. of fuel.

Saving of Time.—The saving of time does not merely amount to what is consequent on the speedier decomposition of the coal, and the saving of that heat which formerly required to be kept up a length of time to no adequate purpose ; it also includes all that is gained in consequence of the revolving motion to which the coal is submitted, superseding, as has been already mentioned, the necessity of discharging the coke in an ignited state from the retort.

When the coke is removed red-hot from the cylindrical, parallelopipedal, semi-cylindrical, or ellipsoidal retorts, the charging of the distillatory vessel with fresh coal produces such a sudden reduction of temperature that from three to four hours inevitably elapse before the retort is again in a full working state, and to this circumstance the workmen (perhaps very justly) attribute the frequent sudden injury which the distillatory cast-iron vessel sustains.

Another striking advantage of the new mode of decomposing coal is, that besides saving the time which is wasted in keeping up an intense temperature unnecessarily the revolving apparatus prevents entirely the loss occasioned by these three or four hours of unnecessary cooling of the distillatory vessel. For each series of trays, or coal-boxes, containing the ignited coke, of the horizontal rotary retort, being suffered to cool within the retort before the coke is discharged, and being placed in contact with a fresh supply of coal, the temperature of the retort is kept up uniformly the same from beginning to end.

Saving of Labour.—In consequence of the superior facility with which the mode of decomposing coal in thin layers and removing the coke as fast as it is formed is effected, the saving in point of labour is very great. The charging and discharging of the retort is performed in two minutes. Hence one chaldron of coal may be decomposed by means of three horizontal rotary retorts, each 12 feet 6 inches in diameter, and with the attendance of two men, in eight hours, and produces from 15,000 to 18,000 cubic feet of gas ; while 10,000 cubic feet of gas can only be obtained from the same quantity of coal in eight hours by means of 20 cylindrical retorts, attended by the same number of workmen.

Saving of Machinery.—When we compare the original cost and wear and tear of the horizontal rotary retorts, with the cost and deterioration of a set of cylindrical, parallelopipedal, ellipsoidal, or semi-cylindrical retorts of an equal power (that is to say, to produce a like quantity of gas in a given time), a difference not less striking presents itself in favour of the horizontal retort.

We have stated already that cylindrical, ellipsoidal, parallelopipedal, or semi-cylindrical retorts, when constantly kept in action, and worked to the greatest advantage, cannot be made to last longer than six months.

Only one-third part of the top and bottom plates of the rotary retort being exposed to the action of heat are alone liable to deterioration. It is only necessary, therefore, that these parts of the vessel be renewed, while the other parts remain uninjured for years. The new top and bottom plates being rivetted to the old and undecayed part, without deranging the rest, the retort is rendered as good as new.

Gain in the Quantity of Gas.—A large increase in the quantity of gas obtained is a natural consequence of the mode in which the decomposition of coal is effected by means of the horizontal rotary retort.

Every body knows that coal, when decomposed slowly, affords a larger quantity of tar and ammoniacal liquor, but a less quantity of gas than when decomposed rapidly.

In the former case, the formation of the proximate products which coal is capable of furnishing is effected properly ; the bituminous part of the coal is developed under the most favourable circumstances.

But when coal, after being previously deprived of moisture, is very suddenly heated to a high temperature, in thin strata, and small portions at a time, so that the vapourous products, instead of becoming condensed, are made to come into contact with a substance (which in this case is the roof of the retort) kept constantly at a temperature rather higher than that at which gold, silver, and copper melts (32° Wedgewood, or 5237° Fahrenheit), a very different arrangement of principles takes place.

The greatest portion of tar which the coal is capable of furnishing, instead of being produced in a liquid form, becomes then decomposed into carburetted hydrogen and olefiant gas. That portion of tar which escapes decomposition is condensed in the perpendicular pipe, H, fig. 2, and falls back again into the retort, where it is also decomposed when the coal upon which it falls comes under the process of decomposition.

Hence the quantity of tar obtained by means of horizontal rotary retorts is very small ; it seldom exceeds the proportion before-mentioned, when the retort is worked to the greatest advantage. This quantity is considerably diminished, when Newcastle coal, broken into pieces of the size of split pease, is decomposed in strata, not exceeding two inches in thickness. The quantity of tar afforded by a chaldron of coal then amounts to 30 lbs. ; while, at the same time, the quality of the gas is improved ; because coal tar furnishes olefiant gas, which the coal alone, when distilled by means of cylindrical or other shaped cast-iron retorts of the usual form, cannot produce, or at least but in a small quantity. One gallon of coal tar yields 15 cubic feet of olefiant gas, which greatly increases the illuminating power of the carburetted hydrogen.

From what has been so far stated, it will be understood why one chaldron of Newcastle coal, when decomposed by the new process, may readily be made to produce from 15,000 to 18,000 cubic feet of gas and upwards, whereas the same quantity of coal, if decomposed by the old method, yields only upon an average 10,000 cubic feet of gas.

In the former case, the greater part of the essential oil and tar which the coal would have afforded is decomposed, as stated already, by virtue of the high temperature to which the vapourous tar is suddenly exposed in the horizontal rotary retort, which is not the case when coal is decomposed in the retorts of the old construction.

Gain in the Quantity of Coke.—With the cylindrical or cast-iron retorts of the old shapes, the quantity of coke obtained from a given quantity of coal is upon an average 25 per cent. increase by measure from the best kind of Newcastle and Sunderland coal ; but taking into account the waste incurred in breaking out and removing the red-hot coke from the retort, which requires the application of rakers and crow bars, a considerable portion of it becomes reduced to dust or breeze, and hence no more than bulk for bulk of the coal decomposed can seldom be depended upon as the ultimate saleable quantity of coke.

In the new mode of carbonizing coal by means of the horizontal rotary retorts, the increase of coke is 150 per cent. by measure ; so that one chaldron of Newcastle coal produces two and a half chaldron of coke—this is the quantity produced upon an average. But when the retort is worked at a temperature to produce at the rate of 18,000 cubic feet of gas from the chaldron of coal, the increase of coke by measure is 175 per cent. ; in that case, the layers of coal in the coal-boxes must not exceed two inches in thickness ; so that the volume of coke is in the ratio of the quantity of gas produced, and the rapidity and elevation of temperature at which the decomposition of the coal is effected.

The coke being withdrawn from the place where it is formed by merely turning the boxes containing it, upside down, all waste is avoided.

With respect again to the quality of the coke, it will be observed that when the coal is rapidly carbonized in thin layers, and has full liberty to expand freely, as in the case of the horizontal rotary retort, it affords a light and porous coke, whereas in the cylindrical, parallelopipedal, semi-cylindrical, or ellipsoidal retorts, the coke being compressed, the intense heat to which it is so long and *superfluously exposed*, renders it extremely dense, and of a stony hardness.

The latter sort of coke is unquestionably preferable for the smelter, and all furnace operations, standing the blast of the bellows well. But the coke produced in the new mode of operating is better suited for the great majority of domestic purposes, kindling more readily, and making a more cheerful fire. The combustion of the dense, or as it is now called, cylinder coke, can be only kept up when used in a common grate, by a strong draft of air ; and it is, therefore, not so well suited for fuel for domestic purposes, to make a small fire ; but the coke obtained by the horizontal rotary retort readily maintains its own combustion, even when in small masses ; hence it may be used without any trouble, either in the fire-place of the cottager, or of the prince, and accordingly it bears a higher price in the market.

I may venture to suggest a suspicion that the gas obtained by this apparatus will yield less light than when it is obtained in the usual way. My reason for the suspicion is the quantity of tar decomposed by it. Now in all my own trials to decompose the tar, the gas obtained burned very badly.

The object of the eighth part is to explain the contrivances fallen upon to separate the sulphuretted hydrogen gas from the coal. This is done by causing the gas to pass through a milk of lime ; and about two per cent. of lime to the gas produced has been found sufficient to purify it when properly applied ; for the ingenious contrivances that have been adopted to ensure the purification of the gas, I must refer the reader to the book itself, as, in order to be understood, they would require to be elucidated by plates.

The ninth part is occupied with descriptions of the gasholders, and with the very ingenious improvements of Mr. Clegg upon this most essential part of the apparatus. His governor, his reciprocating safety valve, and his various modifications of the gasholder, deserve the particular attention of every person who intends constructing an apparatus for procuring light by means of gas.

The tenth part is employed in describing the gas meter, a very ingenious contrivance of Mr. Clegg, by which the quantity of gas formed and the quantity given out to the consumer may be accurately measured. I can only notice the subjects treated of. From the nature of each it would be impossible to convey an accurate notion of them to the reader without transcribing the descriptions and giving the plates ; that is to say, without giving almost the whole of the book.

The remaining part of the book is employed in describing the regulating gauge, the gas mains and branch pipes, the gas lamps and burners, the illuminating power of the gas, and the quantity of it consumed in a given time. To give the reader an idea of this very essential datum, we quote the following paragraph :

The following statement exhibits the quantity of coal gas consumed in a given time by different kinds of argand lamps. An argand burner which measures in the upper rim half an inch in diameter, between the holes from which the gas issues, when furnished with five apertures one twenty-fifth part of an inch in diameter, consumes two cubic feet of gas in an hour, when the gas flame is one and a half inch high. The illuminating power produced by this burner is equal to three tallow candles eight in the pound.

An argand burner, three quarters of an inch in diameter between the holes in the upper rim, and perforated with holes, one-thirtieth of an inch in diameter, con-

sumes three cubic feet of gas in an hour, when the flame is 2½ inches high, and produces a light equal in intensity to four tallow candles, eight in a pound.

An argand burner, seven-eighths of an inch in diameter, perforated with 18 holes- one thirty-second of an inch in diameter, consumes, when the flame of the gas is three inches high, four cubic feet of gas in an hour, and produces a light equal in intensity to six tallow candles eight in the pound.

When the flame obtained by these kind of burners rises to a greater height than what has been stated, the combustion of the gas is imperfect, the intensity of the light becomes diminished, and there is a waste of gas. The same holds good with regard to the size of the holes from which the gas issues. If the holes be made larger than one twenty-fifth part of an inch in this kind of burners, the gas is not completely burned, and its illuminating power decreases.

The height of the glass which surrounds the flame should never be less than five inches, and the interval for the current of air within and without the flame ought to bear the usual proportion adopted for the combustion of oil in the common argand lamps of similar diameters.

We have likewise an account of the mode of obtaining gas from coal tar, and gas from oil. The processes for distilling coal oil, and making carbonate and muriate of ammonia from the ammoniacal liquor, are also given.

ARTICLE IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On account of the death of the King and the Duke of Kent there was no meeting of the Society from Jan. 20 till Feb. 17.

Feb. 17.—At this meeting, Mr. E. Davy's paper, on some Combinations of Platinum, was concluded. The principal object of this paper was to describe a peculiar compound of platinum obtained from the sulphate by the agency of alcohol. Sulphate of platinum was boiled in alcohol; a substance was precipitated; which, when dry, was black, insoluble in water, and unaltered by exposure to air. When heated, it was reduced with a slight explosion. It was insoluble in nitric, sulphuric, and phosphoric acids, but dissolved slowly in the muriatic acid. When steeped in ammonia, it acquired fulminating properties. Alcohol immediately decomposed it, as was shown by slightly moistening it with that fluid; such a heat being produced as to ignite the separated platinum. Hence the author recommended it as a means for procuring an instantaneous light.

When submitted to analysis, it appeared to consist almost entirely of platinum, with a little oxygen, and the elements of nitrous acid. It also contained a small proportion of carbon, which the author considered as accidental. The nitrous acid was supposed by the author to be derived from the sulphate of platinum, this being formed from the sulphuret of platinum by the agency of nitric acid.

In a subsequent part of the paper, Mr. D. described the action of the sulphate of platinum upon jelly, with which it forms a precipitate, and for which, in the author's opinion, it constitutes the best test hitherto known. He then described an oxide of platinum obtained by the action of nitrous acid on fulminating platinum. This oxide is of a grey colour, and, according to his experiments, consists of 100 platinum + 11·9 of oxygen. He considered this as a protoxide composed of 1 atom metal + 1 atom oxygen; while the black oxide of platinum he considered as a compound of 1 atom metal + 1½ oxygen.

Feb. 24.—A paper, by Dr. Wollaston, was read, entitled, "On the Method of cutting Rock Crystal for Micrometers." A wedge of rock crystal, and one of crown glass, may be so combined for the purposes of examining the phenomena of double refraction, that the image of a luminous object seen through them shall appear in its true place by ordinary refraction, accompanied by a second image produced by extraordinary refraction. In consequence, however, of the dispersion of colours, which arises from employing different substances, such a combination is not adapted for the micrometer invented by the Abbé Rochon; but it is not difficult to obtain such a section of rock crystal as may be substituted for the glass wedge, so that the pencil of light shall be colourless without diminishing the separation of the images. But since the distance to which the double refraction of rock crystal separates the two portions of a transmitted ray is sometimes not sufficiently great, it becomes desirable to increase it; and though the means of effecting this have not been published, the author proceeded to describe a method which he found to succeed, and which he regarded as the same as that of M. Rochon. He then described three modes of cutting wedges of rock crystal, so that the axis of crystallization shall be differently situated in each. In the first, or horizontal wedge, the axis is at right angles with the surface. In the second, or lateral wedge, the axis is in the first surface, and parallel to its acute edge. In the third, or vertical wedge, the axis was also in the first surface, but at right angles to the acute edge. Through the first wedge an object is seen in the direction of the axis, and does not appear double; but in both the others, the transmitted rays pass at a right angle to the axis, and they produce two images. By placing two of these wedges together with their acute edges in opposite directions, there are obviously three modes in which they may be combined in pairs. In the first two cases, the separation of the images will be the same, or about 17′; but the third produces a different effect; for, by reason of the transverse position of the axes of crystallization, the separation of the two images seems exactly doubled. The pencil ordinarily refracted by the first wedge is refracted extraordinarily by the second, and *vice versâ*; so that neither of the divided pencils returns to its true place, and since one falls as

much short of the mean as the other exceeds it, they are ultimately
separated twice the usual distance, or to 34'. The paper was
concluded with some further directions for cutting and arranging
the prisms for the above purpose.

March 2.—A paper, by Sir R. Seppings, was begun, on a new
Principle of constructing Ships for the mercantile Navy. In
the present mode of constructing ships, only half the timbers are
united, so as to constitute any part of an arch, every alternate
couple only being connected together; the intermediate two
timbers being connected with, and resting upon, instead of
supporting the outer planking. The mode of joining the diffe-
rent pieces of the same rib are also highly objectionable, this
being at present effected by the introduction of wedge pieces,
by which the grain of the rib piece is much cut, and the general
fabric weakened; besides, there is a great consumption of
materials. The object of these wedge pieces, or *choaks*, is to
produce the necessary degree of curvature when crooked timber
is scarce; but the author showed that this curvature might be
equally obtained by a different arrangement of materials, and
with less consumption, of useful timber. After pointing out
several other defects and disadvantages attending the present
mode of constructing mercantile ships, the author considered the
best modes of obviating them. He recommended that shorter
lengths of timber and of less curvature (which will have the
advantage of being less grain cut), should be employed, and that
these pieces should be connected at their ends by coaks, or
dowells, instead of wedge pieces.

The author then proceeded to point out the advantages of
these improvements, and mentioned a report on the subject by
the officers of Woolwich yard, respecting a ship which had been
constructed on the plan in question. One great advantage stated
to attend this plan was, that smaller timber may be employed
than usual in the construction of large ships, an object of the
first importance at present when large timber is become so
scarce. Drawings illustrative of the different points accompa-
nied the paper.

March 9.—A paper was read, by J. A. Ransome, Esq.
on a Peculiarity in the Structure of the Eye of the Balæna
Mysticetus. On removing a portion of sclerotic coat of the eye
of this animal so as to expose one hemisphere of the choroid
coat, the author opened into a large sinus containing a blood-
vessel, which passed forward in the direction of the iris. On the
upper and lower surfaces of the sclerotica are two foramina for
the passage of vessels; on its flat and posterior surface is a hole
for the transmission of the optic nerve; and on each side corres-
ponding with the long diameter of the eye, are two other foramina
with large funnel-shaped mouths, which extend through the
substance of the coat, and terminate at its junction with the
cornea. These foramina were considered by Mr. Hunter as

passages for vessels ; but they convey two muscles, which, from their appearance and office, the author proposed to call *arcuatores corneæ.* These muscles arise from a large retractor muscle, and the side of a firm sheath, which encloses the optic nerve, which retractor and its sheath are inserted into the posterior surface of the sclerotica. They then pass through the funnel-shaped mouths of the lateral foramina, as before mentioned, and are inserted tendinous into each side of the long diameter of the cornea, which is eliptical. The author supposes their use to be to adapt the eye of the animal for seeing both in air and water.

March 16.—An abstract of a paper, by M. Charles Dupin, was read, on the Laws of the Variation of the Flexibility of Canadian Fir Timber. It has been shown by Duhamel and others that the resistance of timber against bending or breaking is greater at the root than at the top of the tree ; but the mathematical law of this diminution in the strength of the tree from below upwards has not been ascertained. The object of the author was to investigate this point, and with this view he instituted in 1816 a set of experiments on the subject in the dockyards of Dunkirk. The experiments were made upon prisms 50 feet long and one foot thick, and appear to have been conducted with great care ; but the nature of the results, and the mathematical reasoning founded upon them, did not admit of being detailed.

LINNÆAN SOCIETY.

Jan. 18.—A paper was read, by the Rev. R. Sheppard, on the British Fresh Water Mytili.

Feb. 1 and 15.—The Society adjourned.

Feb. 22.—A paper, by Dr. Leach, was read, on Four New Genera of Vespertilionidæ.

At this meeting also, Prof. Temminck's paper, on some Birds from New Holland in the Society's Museum, was continued.

March 7.—Some Remarks on the Shoveller Duck were read, by Mr. Youell, of Yarmouth.

March 21.—Professor Temminck's paper, on the Birds from New Holland, and Dr. Leach's paper, on the Vespertilionidæ, were concluded.

A letter, from W. Butler, Esq. was also read, on the Management of Bees.

GEOLOGICAL SOCIETY.

Jan. 21.—The continuation of a paper was read, " On the Coal Fields adjacent to the Severn ; " by Professor Buckland and Rev. W. D. Conybeare.

From the northern apex at Tortworth where the west frontier of the coal field meets its east border at an acute angle, the latter is continuous with its usual character as far as Sodbury ; it *is here, however, so low, that having been partially overrun by*

lias and red ground between this place and Tortworth, it now
becomes totally so from hence to the east extremity of Mendip,
with the exception of the three small distant spots at Lodming-
ton Court, Wick Rocks, and Tracey Park, where a denudation
of the lias and red ground *exposes to view* the subjacent continua-
tions of the calcareous border of the coal basin in the direct line
between Sodbury and the east extremity of Mendip.   The
remainder of the calcareous border is completely buried under
strata of oolite and lias, and the young red sandstone in the
interval between Tracey Park and Mells; and immediately
within it, shafts are frequently sunk through horizontal beds of
lias and red ground to obtain coal, which is usually dipping at a
high angle, unconformably to the more recent beds that cover it.
    The importance of this district in demonstrating the relations
of the coal measures to the young red sandstone formation,
which have hitherto been so little understood, must be at once
obvious.
    It is probable that the whole area of the South Gloucester and
Somerset coal field was at one time entirely buried by beds of
lias and young red sandstone, and that where the coal measures
are now visible at the surface is in consequence of the removal
of the strata by denudation.
    A large proportion of the Somerset collieries are won by
shafts beginning in lias or red ground; so also is that of Puckle-
church on the border of Kingswood coal field.
    The points within the limestone border, at which the coal
measures occupy the surface, and seem at first sight to consti-
tute distinct coal fields, are five in number: as they are considered
to be laid open in consequence of the removal by water of their
horizontal coverings, they are described as so many denudations
made up of the central or Pensfold denudation; northern, or
Kingswood ditto; southern, or Nettlebridge ditto; and eastern,
or Wick and Newton St. Loo denudation. Of these, the north-
ern is the largest, its greatest breadth being four miles; these
five masses of denuded strata occupy usually the lowest grounds;
and the intervening hills are composed of horizontal strata of
young red sandstone, lias, and occasionally of oolite. The atten-
dant coal fields of Nailsea appear also to be exposed in conse-
quence of a similar denudation.
    The strata, whose aggregate composes the entire coal field of
South Gloucester and Somerset, are reducible to three leading
subdivisions.
    1. Lowest, or Brandon Hill Grit, being the same with the
Millstone Grit of Derbyshire; it is best displayed at Brandon
Hill, near Bristol, and at the village of Clifton.
    2. Lower coal seems alternating with beds of grit and shale,
in which the shale largely predominates.
    3. Upper coal seems alternating with a few beds of shale, and
thick beds of gritty freestone, or Pennant stone. All these coal

measures dip on every side from the circumference to the interior of the basin in which they occur. A similar subdivision extends through the coal basins of the forest of Dean and South Wales, a description of which will be next entered upon in conformity with the outline given in the introductory paper, or general view of the several coal fields in the districts bordering on the Severn, of which the Somerset and South Gloucester coal basin form a part ; this paper, containing a general view of these several coal basins, and of the intermediate districts on both sides of the Severn, together with a coloured map and section of the whole area, was laid before the Geological Society in Nov. 1818.

A paper, by the Rev. James Yates, of Birmingham, was read, giving "An Account of a Variety of Limestone found in Connexion with the Clay Ironstone of Staffordshire."

This peculiar ironstone has obtained the provincial name of *Curl*, from its peculiar figure, as consisting of concretions which, when united together, approach more or less to the conical form. From its external shape bearing some resemblance to the leaves of a palm folded over each other, it was formerly conceived to be of vegetable origin ; but the author observes that this opinion is evidently incorrect. The curl is always found in connexion with the bed of clay ironstone, in the south-east of Staffordshire, which is termed the *bottom stone of the new mines*.

The author describes the substance in question, as well as the strata to which it is attached ; it generally is found contiguous to the under surface of the ironstone, and is connected with it by the apex of the cone ; but the author suspects that in some situations it may be above this stratum of ironstone, but still with its apex towards the stratum.

The curl occurs in large masses, forming protuberances from 5 to 20 yards in extent, and from four inches to half a yard in thickness. It is firmly attached to the ironstone, and would seem to be of contemporaneous formation.

The paper was accompanied by specimens.

March 3.—A letter from M. de la Beche, dated Geneva, was read, informing the Society that he was sending some specimens of alpine rocks, and mentioning that in the Museum of Natural History at Geneva he had found several fossils from the blue lias of Havre, in France, which correspond exactly with those of the blue lias at Lyme, in Dorsetshire ; amongst them were vertebræ of the ichthyosaurus (the proteo-saurus of Sir E. Home), which appear to be more rare in the lias of France than in that of England ; but in the former, the remains of a fossil crocodile have been found.

An Analysis of the Labours of the Royal Academy of Sciences during the Year 1818.

(*Continued from* p. 224.)

New Method of laying the Strands of Cordage, proposed by M. Duboul, Master Ropemaker for the Merchant Service at Bordeaux.—Committee, Messrs. Girard, Molard, Sané, and Dupin, Secretary.

To *lay* the strands of a rope is to put together and unite by twisting, the elements of the rope which they call *strands*, and which are themselves formed of other srlands twisted together, or of simple threads uniformly twisted. Duhamel-Dumonceau made a number of experiments in our dock-yards in order to appreciate the different methods in use, which had been discovered by frequent trials, and preserved by custom. The only fault that can be found with him is that of having employed clumsy machinery, which did not allow him to attain a sufficient degree of precision. Dynamometers have been constructed some years past, upon the plan of M. Hubert, which serve to determine the strength of hemp. The torsion which they can produce, however, does not go beyond 2000 kilogrammes (one ton); it is necessary that they should be made to produce a torsion of 100,000 kilogrammes (50 tons), similar to those which have been constructed in England, to try the strength of iron cables and those of hemp. M. Marestier is the first person in France who resolved the problem of twining hemp, and laying strands of an indefinite length in a limited space. M Chanot afterwards resolved the same problem ; and by a remarkable chance, his solution of it was found to be the same with that of M. Marestier in every important part of the mechanism. M. Hubert has been able to comb hemps in a very expeditious and regular manner, and to twine them with a very light wheel, without any change of position either of the wheel, or of the workman. Lastly, Col. Lair has just perfected the laying down of cables by equalizing the strain that is necessary to hinder the strands of the cable from twisting too quick, before the strands themselves have attained the most advantageous twist. M. Duboul, who has already shown a very remarkable talent in workmanship, now appears as the author of several methods of increasing the strength of cordage. In general, he twines his cordage less, and twists the simple strands more in proportion ; afterwards, he twists less the first, and still less the second laying down of the strands. The Committee discussed the inconveniences and advantages of these innovations, and they think that the new proportions given by M. Duboul merit due examination, and that they ought to be tried with a well-made set of *comparative* experiments. The two machines that M. Duboul

proposes for laying down the strands have, it is true, no novelty in a mechanical point of view; but this does not render them less useful in respect to the making of ropes. The conclusion drawn from the report is, that the perseverance exhibited by M. Duboul in endeavouring to improve his manufacture, and the expense he is at for that purpose, merit the highest praise; that his two machines, costing but little, may, in many cases, be of use in the rope yards; and the different degrees of twist proposed by M. Duboul offer sufficient advantage in theory to merit trial and examination with all the care and zeal that mechanics, who are friends to the progress of the arts, can bestow upon them.

Lamp of Messrs. Gagneau and Brunet.—Committee, Messrs. Gay-Lussac, Thenard, and Charles, Secretary.

The use of lamps with a double current of air is become so universal, that it forms in itself a very considerable branch of manufacture, which is continually increasing. New forms and new compositions are invented every day. Amongst these various kinds of lamps, many of which answer their purpose very well, there is one in particular that has for 20 years past constantly remained superior to the rest, both with regard to the brilliancy of its light and the regularity of its action; namely, Carcel's. But however perfect this lamp may be, the expense of its workmanship, the delicacy of its construction, and the still greater difficulty of repairing it, rendered it desirable that intelligent artists should modify this lamp in such a way as, while it preserved all its advantages, its mechanism might be simplified, and its execution, and especially its repair, be facilitated. Such is the lamp of Messrs. Gagneau and Brunet. Upon a comparative trial with one of the best of Carcel's for three successive nights, it preserved its equality during 10 hours. The new lamp will even burn 12 hours; but this length of time, to which the spring will extend, is nearly superfluous, as, at the end of 10 hours, the wick is burned to a coal. The duration of the wick depends in some measure on the more or less capillary quality of the cotton of which it is formed; but that duration depends still more on the goodness of the oil; in the experiments which were made, the best oil was always employed. In an inverted application which they have made of the pump, known for these hundred years past by the name of *le pompe des prêtres,* the authors have succeeded in substituting two diaphragms of oiled silk for Carcel's pump, and the friction being reduced nearly to nothing, i tallowed of their omitting two wheels, of lessening the force of the moving spring, and nevertheless of raising the oil to a greater height. The introduction of a reservoir of air renders this elevation constant and uniform; but in Carcel's lamp it is intermitting, like the strokes of a piston. The power of giving to the bottom of the lamp and the column a more light and slender form is also a very agreeable improvement. Another advantage is, the ease with which it may be repaired, whenever the diaphragms require

to be renewed. But a very important difference, and one which will be favourably received by the public, is the diminution of the price. From all these circumstances, we think it right to infer, that the lamp deserves the approbation of the Academy.

A Treatise upon Wheels for raising Water; by M. Navier.— Committee, Messrs. de Prony, Fourier, and Dupin, Secretary.

M. Navier undertakes to determine the proportion between the *vis motrix* and the effect produced in rotatory machines for raising water.

The principle of the preservation of the *vis viva* gives a mathematical relation between the four species of forces that remain to be considered in the problem, if we neglect the friction and the cohesion of the water, which indeed are very little. This principle, which was discovered by Huygens, was placed by John Bernoulli in the number of the fundamental laws of dynamics; Daniel made some fortunate applications of it; and Borda used it with great success in the calculations of several machines of which water was the moving power. In those which M. Navier notices, it is, on the contrary, the water which is raised by some other foreign power. We owe to Borda the first exact calculation of the *vis viva* lost, but he only gave it for particular cases. To M. Carnot we are indebted for the general law which he has clothed in the following theorem. " In every case of a body in motion, passing from one situation to another, the sum of the quantities of action which have been during this interval impressed by all the forces is always numerically equal to half the sum of the *vis viva* acquired at the same time by the different bodies in question, plus the half of the *vis viva* lost by the effect of the sudden changes of velocity, if there have been such changes."

Wheels for raising water are divided into three classes, according as the rotatory axis is horizontal, vertical, or inclined.

In the bucket wheel there is a *vis viva* acquired by the water at the instant one of the buckets is filled, and another lost at the instant it is emptied. From the above-mentioned law may be gained the proportion of the *vis motrix* to the effect of the machine; and by a simple differentiation, we obtain the velocity that gives the most favourable proportion.

In the drum wheel there is not any power lost; hence this wheel is more advantageous than the preceding one.

M. Navier describes very minutely the spiral pump formed by a tube of an uniform or variable size, bent in a spiral on a cone whose axis is horizontal. This ingenious machine has the very important merit of producing more beneficial effects in proportion as it is used in raising water to a greater height. A calculation of M. Navier fixes the height at which that property begins to be very perceptible.

If we fasten to a vertical axis a syphon inclined in such a manner as to rise in a contrary direction to the rotary motion,

the lower end being plunged into water, the water will rise by the effect of the rotation. The author also calculates the effect of a machine formed of two paraboloids, turning together on the same vertical axis, and united together by inclined partitions.

Archimedes's screws form that kind in which the axis is inclined. Daniel Bournoulli devoted some attention to their theory, but he did not exhaust the subject, as M. Navier has done. In respect to the case in which a pipe of an uniform diameter, bent spirally on a cylinder whose axis is inclined, is filled alternately with water and with air, he demonstrates in a simple and elegant manner that the surface of the water must be a paraboloid, having the axis of the cylinder for one of its diameters, and the surface of the water at rest for a tangent plane at the extremity of the diameter.

For the common screw, formed by the revolutions of an internal screw, in a circular cylinder, after having sought the quantities of water contained in each turn of the screw, he draws up tables to shorten the necessary calculations, according as the turns of the screw are more or less near, and their axis more or less inclined.

The very extensive work of which we have just given an account, say the Committee, appears to us to be of the number of those which the Academy ought, by its approbation, more especially to encourage. To extend by an uniform progress the theoretical methods of appreciating the effects of machines is to narrow gradually the circle of empiricism : it is to furnish artists with general means of becoming acquainted with the advantages and disadvantages they may hope or fear from their inventions.

The Academy has, in consequence, ordered, that the treatise of M. Navier should be printed in the next volume of the Savans-Etrangers.

(To be continued.)

ARTICLE X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Boiling Springs in the Island of St. Lucie.*

THE face of the country is extremely rugged, and intersected in all directions by high pointed hills. In one place there still is a curious phenomenon. At the head of an extensive valley, there are situated a number of boiling springs, the number of which varies at different times, but generally eight or ten of them discharge water at the same time. This, however, is quite uncertain, as they frequently dry up, and again burst forth, and flow with much violence. The ebullition of some of

them is so strong that, independently of the aqueous vapour formed by the heat, and which issues· from the apertures, a great quantity of sulphureous air is emitted, and the black muddy substance contained in the basins is thrown up as high as seven or eight feet. The efflux of water from the springs is very scanty, and in some of them the whole supply is converted into vapour by the subterranean heat.

After a long course of dry weather, these springs are almost wholly dried up, and when rain falls, they boil out again with redoubled fury.

There are large mounds of finely crystallized sulphur in the vicinity of these springs, and quantities of a white earthy substance are also found in their neighbourhood.

The colour of the liquid discharged by the springs is very various ; and what renders this remarkable is, that some of them are situated within a yard of each other, and throw out water of a different colour ; in one only it is limpid; in the rest it varies from a milky whiteness to a thick dark-black. The hills in the neighbourhood of these springs are high, and some of them bear evident marks of having formerly been the seats of volcanic eruptions.

II. *Singular Calculi said to be from the Urinary Bladder of a Dog.*

A gentleman of great respectability from Canada, and a member of the Colonial Assembly at Quebec, lately presented a number of round bodies to the Hunterian Museum in Glasgow, which he had received from a person residing on the banks of the river St. Laurence. This person assured him that they had been taken after death out of the urinary bladder of a dog.

As soon as I saw these bodies, I suspected them to be *pearls.* They are perfectly spherical, about the size of mustard seed; they have the lustre and the weight of pearls ; but their colour is not good, being rather dark, and inclining to yellow. Like pearls, they are composed of very thin concentric coats ; and I found them composed of lime united to an animal matter. Thus in their composition, as well as their appearance, they agree with pearls. Pearls indeed, according to the analysis of Mr. Hatchett, are composed of carbonate of lime and an animal matter. When I put one of these bodies into nitric acid, it dissolved slowly, but completely; and I did not perceive any sensible effervescence. Here then appears a difference between our concretions and pearls. But my experiment was made upon so small a scale, and the solution was so slow, that I consider the difference to be only apparent and not real. The matter dissolved by the nitric acid was pure lime ; for it was not precipitated by pure ammonia; but was readily precipitated by oxalic acid. I think, therefore, that it probably exists in the concretions in the state of carbonate of lime.

As to the truth of the assertion that these concretions were actually taken out of the urinary bladder of a dog, there seems to me to be some room for scepticism. The gentleman who presented the concretions to the museum is a man of the highest respectability, and he himself firmly believed the truth of the statement which he gave. He must, therefore, have entertained a favourable opinion of the veracity of the person from whom he received them. There is no doubt a possibility that he may have been imposed upon ; though for what purpose such a deception could have been practised, in a case where neither profit nor credit could redound from it, and where the person practising it did not even seem to have been aware that such concretions were extraordinary, it is difficult to conceive.

The thing seemed to me at least of sufficient importance to deserve to be recorded. It may serve to draw the attention of others to such concretions, if they should ever again occur.

III. *Naphtha from Persia.*

Mineralogists and chemists are aware of the existence of naphtha in Persia, and of the many wonderful stories that have been related of its volatility and combustibility. I have been lately favoured, through the kindness of a gentleman who has spent many years in the neighbourhood of Persia, with a specimen of the naphtha in the purest state in which it occurs. It is colourless as water, has the specific gravity 0·753, and precisely the same smell and taste as the naphtha which is made in this country from the distillation of coal. Indeed our artificial naphtha and the Persian naphtha resemble each other in all their chemical properties as far as I have compared them together. I have never got any naphtha made in this country from coal quite so light as the Persian. The specific gravity of the lowest which I have met with was 0·817, but probably had it been rectified once or twice more, it would have become as light as the Persian.

The statements respecting the extreme volatility of naphtha have not been confirmed by my experiments. The Persian naphtha boils when heated to 320°. If we continue the boiling, the naphtha becomes darker coloured, and the temperature may be made to rise as high as 338°, and perhaps even higher. Indeed in a silver vessel I raised its temperature to 352°. The same increase of temperature takes place when oil of turpentine is kept boiling. There are two consequences which may be drawn from these facts ; and one or other of them must be the true one. Either naphtha and oil of turpentine are composed of two distinct liquids differing in their volatility ; or they are partially decomposed at the boiling temperature. From the increase of colour which takes place when naphtha is boiled, one would be disposed to adopt the second of the two alternatives.

When a grain of Persian naphtha is decomposed in the usual way by means of peroxide of copper, we obtain 1·35 grain of

water and 6·5 cubic inches of carbonic acid gas. Now the hydrogen in 1·35 gr. of water is very nearly equal to seven cubic inches. The carbon in 6¼ cubic inches of carbonic acid is equivalent to 6¼ cubic inches. Hence it follows that naphtha is composed of

6¼ or 13 volumes of carbon
7 or 14 volumes of hydrogen

By substituting atoms for volumes, which may be done in this case without any error, it follows that naphtha is a compound of

13 atoms carbon .... = 9·75
14 atoms hydrogen .... = 1·75
                                  ─────
                                  11·50

The specific gravity of the vapour of carbon is 0·416, and that of hydrogen gas 0·0694. Therefore,

6¼ cubic inches of carbon weigh .. 0·822 gr.
7 cubic inches of hydrogen weigh 0 148
                                 ─────
                                 0·970

There is, therefore, in this analysis, a deficiency of three per cent. I am disposed to ascribe this to a small portion of azote, which naphtha seems to contain. But I have not been able to satisfy myself experimentally of its existence. My experiments are conducted in copper tubes, subjected to a red heat. This always drives off a quantity of air, varying from 0·5 to 0·7 of a cubic inch, according to the degree of heat to which the tube and the peroxide of copper is subjected, and which it is not possible to raise always to the same degree of intensity. This air always contains 3¼d per cent. of oxygen, the rest being azote. The reason of this difference between its composition and that of common air is the length of red-hot copper tube through which it is obliged to pass, and which is partially oxidized at the expense of the oxygen of the common air present. Now 0·03 gr. of azotic gas would not amount to $\frac{1}{10}$th of a cubic inch, which, being less than the variation in the quantity of air driven off by heat when nothing is heated but the tube filled with oxide of copper, I have no means of determining whether so small a quantity of azote is disengaged or not.

I have observed of late, that in order to ensure accuracy in the quantity of water evolved, it is necessary to expose the peroxide of copper to a red heat just before making the experiment; for peroxide of copper has the property of imbibing a little water from the atmosphere, which it gives out again when heated to redness.

In order to ensure absolute precision in such experiments, it would be necessary to have the means of raising the fire every time to exactly the same intensity. It is likewise necessary to

have always the very same bulk of peroxide of copper, and of muriate of lime, in the tubes. When these precautions are not attended to, the quantity of common air evolved varies so much as to baffle all attempts to determine the quantity of azote given out, unless it be very considerable. These niceties would be of very little consequence if we could decompose quantities of the substances subjected to experiment amounting to 10 grs.; but this I have not hitherto been able to accomplish. I mix the grain of naphtha with a little peroxide of copper, and let it down into the bottom of the tube. By surrounding it with moist clay, I keep it cool till the further extremity of the tube is red-hot. I then remove the clay, and allow the bottom of the tube to become hot enough to volatilize the naphtha completely. Should any ammonia or nitric acid be formed, they would be decomposed while passing through at least 12 inches of red-hot peroxide of copper.

IV. *Mount Canigou.*

Canigou is a celebrated mountain in the chain of the Pyrenees, situated in the part of France formerly called Rousillon, and at present the department of the *Pyrenees Orientales.* According to the measurement of M. Mechain, it is 1431 French toises, or 9150 English feet, above the level of the sea. It was long considered as the highest mountain in the Pyrenees; though it is now known that several exist in that extensive chain which surpass it in heat by at least 2000 feet. This mountain lies west, and a little south from Marseilles, at the distance of 57 French leagues; yet at certain seasons of the year it is perfectly visible from that city. Baron von Zach, while living in Marseilles in 808, resolved to verify this assertion, which had been repeated him by so many eye witnesses of the fact, that he had no reason to disbelieve it. He considered it as likely that the mountain would be visible only at those seasons of the year when the sun set directly behind it. This he found to happen about the beginning of February and the end of March. Accordingly on Feb. 8, 1808, he went towards sunset to the top of the mountain Notre Dame de la Garde, accompanied by M. Thulis, M. D'Aubuisson, M. Reboul, and M. Martin; and provided with the requisite instruments to observe the mountain. As soon as the sun was set, the mountain, and various other peaks, appeared so distinctly that the spectators could hardly persuade themselves that it was the Pyrenees which they saw; but rather some mountains in the neighbourhood of Marseilles.

Baron von Zach found that the azimuth (or the angle with the line joining the Canigou and Marseilles makes with the meridian) of the highest peak of the Canigou from the place where he was placed was 71° 20′ 8″ south of west; while that of Mont Ventoux, near Avignon, was 4° 17′ 27″ north of west. —(*Correspondence Astronomique,* i. 413.)

V. *Mont Ventour.*

This mountain is remarkable for the suddenness of its elevation above the surrounding country. It is of limestone ; and its height, as determined by Baron von Zach, is 985¼ French toises, or 6301 English feet, above the level of the sea. It is certainly, therefore, one of the highest limestone mountains which are known to exist. The usual calcareous petrefactions abound on the south side of it ; but it is said that none are to be found ou the north side. If this be true, it is a very remarkable phenomenon indeed, and seems hardly susceptible of explanation, unless the petrefactions be confined to the surface of the rock. In that case we are at liberty to suppose that there was a time when the sea washed its south side, while the north side must be supposed to have been always screened from the action of that element. The mountain is covered with angular fragments of a very compact limestone.

Petrarch was partly educated at the foot of this mountain at Carpentras. He ascended it in 1345 with his brother Gerard, and on his return wrote a spirited account of his expedition in a letter to his friend John Colonna, which is to be found among his Epistolæ de Rebus Familiaribus. The latitude and longitude of this mountain as determined by Baron von Zach is as follows :

Latitude . . . . . . 44° 10′ 27·6″ N.
Longitude . . . . 22 56 34·4   E. from Ferro.

(Ibid. p. 426.)

VI. *Positions of various Places in the Coast of the Adriatic Sea.*

Baron von Zach gives the following geographical positions of various places on the coast of the Adriatic Sea. He gives them, he says, with some reserve, and only provisionally. They have been determined by two Neapolitan astronomers, and doubtless from the best data which they could procure : but it does not appear that these astronomers determined any of the positions by observations of their own.

	Longitude.		Latitude.	
Andria	41°	15′	14°	6′
Ascoli	41	8	13	20
Barletta	41	19	14	8
Bari	41	8	14	47
Brindisi	40	40	16	1
Chieti	40	20	11	55
Gallipoli	41	3	16	5
Lanciano	42	12	12	9
Lecce	40	22	16	17
Manfredonia	41	39	13	43
Molfetta	41	13	14	27
Napoli	40	58	15	15
Taranto	40	8	16	39

	Longitude.			Latitude.	
Pescara.	42°	26′	......	11°	56′
Trani.	41	17	......	14	15
Vasti.	42	7	......	12	25
Viesti.	41	56	......	12	55

(Ibid. p. 463.)

VII. *Brucine.*

The discovery of the alkaline properties of *morphia*, for which we are indebted to Sertürner, has drawn the attention of chemists to the discovery of vegetable substances capable of neutralizing acids, and of course possessed of alkaline properties. MM. Pelletier and Caventou have particularly distinguished themselves in these researches. Our readers are already aware of the discovery of two new vegetable alkalies, to which the names of *vauqueline* and *strychnine* have been given. Pelletier and Caventou have lately discovered another, to which they have given the name of *Brucine*, from Mr. Bruce, the Abyssinian traveller, who first made known the tree from the bark of which the new alkaline substance is obtained. This bark is known by the name of *false angustura.*

To obtain *brucine*, the bark of the false angustura (*brucea antidysentericus*) was treated in the first place with sulphuric ether to get rid of a fatty matter which it contains. It was then subjected to the action of alcohol. The alcoholic solutions were evaporated to dryness, the dry residue was dissolved in water, and the solution mixed with subtritacetate of lead (Goulard's extract) which threw down the greatest part of the colouring matter. The excess of lead was got rid of by a current of sulphuretted hydrogen gas. By this means the liquid was rendered nearly, but not completely, colourless. The brucine was not precipitated by ammonia, and all attempts to procure it in a state of purity were long unsuccessful; at last our experimenters succeeded by the following manner: The brucine was saturated with oxalic acid, and the solution evaporated to dryness. The dry mass was digested in absolute alcohol of the temperature of 32°, which dissolved the colouring matter, and left the oxalate of brucine in the state of a fine white powder. The oxalate, when treated with lime or magnesia, is decomposed, and the brucine disengaged. It was dissolved in boiling alcohol, and obtained in crystals by the slow evaporation of the liquid. Thus obtained in a state of purity, it possessed the following properties:

Its crystals, when obtained by slow evaporation, are oblique prisms, the bases of which are parallelograms. When deposited from a saturated solution in boiling water by cooling, it is in bulky plates somewhat similar to boracic acid in appearance. The alcoholic solutions are apt to deposit it in the form of mushrooms. In the last two states, it is very bulky, retaining a great deal of water, which may be forced out by compression. It then diminishes very much in volume.

Brucine is soluble in 500 times its weight of boiling water, and in 850 times its weight of cold water. The colouring matter with which it is united in the bark increases its solubility very much. Hence the difficulty of purifying it by crystallization.

Its taste is exceedingly bitter and acrid, and continues long in the mouth. When administered in doses of a few grains, it is poisonous, and acts upon animals in the same way as strychnine, but with a great deal less energy.

It is not altered by exposure to the air: it may be melted by heat without undergoing decomposition, and then assumes the appearance of wax. It melts at a temperature a little higher than the boiling point of water. The crystals deposited from alcohol sometimes melt at a much lower temperature; but this anomaly is owing to a portion of alcohol retained between the plates of the crystals.

Brucine, when exposed to a strong heat, is decomposed. The products are much empyreumatic oil, a little water and acetic acid, carburetted hydrogen, and a little carbonic acid. No trace of ammonia can be detected among the products. When heated with peroxide of copper, it yields only carbonic acid and water, with scarcely a trace of azote. Hence it can contain only carbon, hydrogen, and perhaps oxygen. But Pelletier and Caventou have not yet succeeded in determining the proportion of its constituents.

It combines with the acids, and forms both neutral and bisalts. Both of these sets of salts, but especially the latter, crystallize with facility.

1. *Sulphate of Brucine.*—Brucine dissolves readily in sulphuric acid, and is capable of forming with it a neutral salt. This salt crystallizes in long slender needles, which appear to be four-sided prisms terminated by pyramids so fine that their shape could not be determined even by employing powerful magnifying glasses. It is very soluble in water, and somewhat soluble in alcohol. Its taste is very bitter. It is decomposed by potash, soda, ammonia, barytes, strontian, lime, and magnesia. It is decomposed likewise by morphia and strychnine, which dissolve readily by uniting to its acid.

No acid is capable of decomposing this salt, except strong nitric acid, which acts upon the brucine, and decomposes it, forming a fine red colour. It produces the same alteration of colour upon strychnine and morphia.

The bisulphate of brucine is less soluble in water, and crystallizes more readily, than the neutral sulphate. The neutral sulphate of brucine, according to the analysis of Pelletier and Caventou, is composed of

Sulphuric acid .... 8·84 ...... 9·697 ...... 5·000
Brucine .... 91·16 ...... 100·000 ...... 51·582
                          ———
                        100·00

This would make the weight of an integrant particle of brucine 51·582, a much higher number than belongs to any other saline base hitherto determined; but no great stress can be laid upon the preceding determination.

2. *Muriate of Brucine.*—Brucine dissolves readily in muriatic acid. The solution yields crystals with facility, the shape of which is a four-sided prism, terminated at each end by an oblique face. It is not altered by exposure to the air, and is very soluble in water. When heated to the temperature at which vegetable bodies begin to be altered, it is decomposed, and the muriatic acid is disengaged in a white smoke. It is decomposed by sulphuric acid. Nitric acid produces the same effect upon it as upon the sulphate. Its constituents are:

Muriatic acid	5·9533	6·331	4·625
Brucine.	94·0467	100·000	73·053
	100·0000		

According to this analysis, the equivalent number for brucine is 73·053, which is nearly one half greater than the number obtained by the analysis of the sulphate of brucine. This prodigious discordance between the results obtained from two different salts demonstrates the inaccuracy of the analyses of Pelletier and Caventou.

3. *Phosphate of Brucine.*—Brucine dissolves readily in phosphoric acid. The neutral salt does not crystallize; but the biphosphate yields large crystals with facility. These crystals are rectangular tables with bevelled edges. The salt is very soluble in water; when exposed to the air they effloresce slightly. In strong alcohol these crystals dissolve with difficulty, and in small quantity. Hence alcohol may be employed to purify the phosphate of brucine by depriving it of its colouring matter, if it has not been got rid of before.

4. *Nitrate of Brucine.*—Neutral nitrate of brucine does not crystallize; but, when evaporated, assumes the form of gum. The binitrate of brucine crystallizes with facility in acicular four-sided prisms, terminated by dihedral summits. When these crystals are heated sufficiently, they catch fire and burn, as is the case with binitrate of strychnine.

When brucine is digested in a still greater quantity of nitric acid, a fine red colour is developed. The same phenomenon appears with strychnine, but the shade of colour is different. When either of these red liquids is heated, it becomes yellow. Protomuriate of tin dropped into the yellow liquid from strychnine occasions a dirty-brown precipitate, whereas in the yellow liquid from brucine, it strikes a very intense and beautiful purple.

5. *Other Salts.*—Acetate of brucine is very soluble, and does not seem capable of crystallizing.

Oxalate of brucine crystallizes in long needles, especially when it contains an excess of acid.

Brucine is very soluble in alcohol; but it is insoluble in sulphuric ether and the fixed oils, and very little soluble in the volatile oils. When administered internally, it produces tetanus, and acts upon the nerves without attacking the brain, or affecting the intellectual faculties. Its intensity is to that of strychnine as 1 to 12.—(See Journ. de Pharm. Dec. 1819, p. 529.)

VIII. *Equivalent Numbers for Morphia, Strychnine, and Brucine.*

Pelletier and Caventou have analysed the sulphates of brucine, strychnine, and morphia, respectively, and found the composition of each as follows :

1. *Sulphate of Brucine.*

Sulphuric acid. ...... 8·84	...... 9·697	...... 5·000
Brucine .... 91·16	...... 100·000	...... 51·582
100·00		

2. *Sulphate of Strychnine.*

Sulphuric acid, ... 9·5	...... 10·486	...... ·5·000
Strychnine .... 90·5	...... 100·000	...... 47·682
100·0		

3. *Sulphate of Morphia.*

Sulphuric acid. ...... 11·084	...... 12·465	...... 5·000
Morphia............ 88·916	...... 100·000	...... 40·112
100·000		

According to these analyses, the equivalent numbers for these substances are the following :

Brucine	51·582
Strychnine	47·682
Morphia	40·112

If the reader will compare these analyses with the analyses of the salts of morphia by Choulant, which will be found in the *Annals of Philosophy,* xiii. 154, he will see what an enormous difference there is between the two results. It is so great indeed as to be quite inexplicable, and prevents the possibility of putting much confidence in either of them. Further researches are requisite before we can expect any precise knowledge of these bodies. The first step ought to be a careful analysis of the bodies themselves. This once known, it would be much easier to determine how much of each is requisite to saturate a determinate quantity of sulphuric acid, which would give us the *equivalent* number for each.

XI. *Bristol Literary and Philosophical Institution.*

On Feb. 29, the ceremony of laying the foundation stone of a new and magnificent building for literary and philosophical purposes in Bristol was attended by the Mayor, W. Fripp, Jun. Esq. the Sheriffs, and a numerous assemblage of gentlemen, some of the most distinguished for wealth and talent in Bristol. The company met their Chief Magistrate at the Council House, and thence proceeded with a band of music, and the insignia of the city, to the ground; and afterward returned in similar procession to the Merchants' Halls to dinner.

The site of this building is at the west end of the bottom of Park-street, one of the finest streets in Bristol. It is intended for the building to " contain a spacious lecture room, with a laboratory adjoining; a room of noble dimension destined for a library ; two apartments which may be appropriated, the one for an exhibition room, the other for a museum ; a reading room for reviews, pamphlets, newspapers, &c.; some other apartments for subsidiary purposes, and accommodation for a resident guardian of the building."

It has been for several years in contemplation to form a philosophical society in Bristol, after the example of London, Edinburgh, Liverpool, Dublin, and some other great towns of the empire; but from the intervention of some causes or other circumstances have continually occurred to delay the execution of so desirable an object. There is now, however, but little doubt, from the zeal which is manifested by the inhabitants of Bristol, for adding so useful an Institution to the city, and so great an ornament to its taste and opulence, that what the friends of this Institution have been so long, so sedulously, and so laudably endeavouring to effect will be attended with the completest success. It is unnecessary to enter into a detail of the advantages to society, commerce, and the arts, which have been uniformly derived in other places from establishments of this kind; they are too familiar to every well informed mind to need any comment or observation. Justice, however, requires it should be known, that the patrons of this Institution have formed their plans upon the broadest basis of enlightened liberality. Besides the cultivation and diffusion of the nobler sciences, and the prosecution of whatever is likely to be of real service or utility to the community and the rising generation, they intend to make this Institution a focus, in which to collect and concentrate, not only the scattered rays of the genius and ability of Bristol, but also of all true lovers of scientific pursuits ; to confine their patronage to no particular branch or branches of sciences, but to extend and afford the utmost encouragement for the development of talent in every department of useful knowledge and literature.

ARTICLE XI.

Astronomical, Magnetical, and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·27″ North. Longitude West in time 1′ 20·93″.

Astronomical Observation.

Immersion of χ Leonis. ...... 10ʰ 25′ 40″ Mean Time at Bushey.

Magnetical Observations, 1820. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Feb. 1	8ʰ 45′	24° 31′ 56″	1ʰ 25′	24° 39′ 45″		
2	8 50	24 33 02	1 15	24 36 14		
3	8 50	24 31 59	1 20	24 37 08		
4	8 45	24 31 19	1 20	24 37 08		
5	8 50	24 32 23	1 50	24 37 25		
6	8 50	24 32 55	1 45	24 36 31		
7	8 50	24 34 11	1 25	24 42 00		
8	8 50	24 33 04	1 20	24 37 13		
9	8 50	24 34 12	1 15	24 38 56		
10	8 45	24 32 18	1 25	24 37 55		
11	8 40	24 32 26	1 25	24 41 14		
12	8 40	24 41 09	1 10	24 36 27		
13	8 45	24 31 54	1 35	24 38 33		
14	8 45	24 31 00	1 25	24 38 14		
15	8 45	24 32 54	1 20	24 40 30		
16	8 40	24 32 53	1 25	24 39 06		
17	8 50	24 32 58	1 20	24 39 13		
18	8 45	24 32 45	1 20	24 38 14		
19	8 40	24 31 29	1 20	24 38 04		
20	—	— —	—	— —		
21	8 50	24 32 31	1 20	24 37 44		
22	8 45	24 31 50	1 25	24 37 56		
23	8 50	24 31 16	1 25	24 38 10		
24	8 45	24 31 12	1 20	24 38 24		
25	8 45	24 31 32	1 25	24 38 07		
26	8 50	24 32 56	1 20	24 37 03		
27	8 40	24 32 57	1 20	24 39 16		
28	8 40	24 31 20	1 25	24 39 12		
29	8 35	24 31 10	1 35	24 36 52		
Mean for Month. }	8 48	24 32 19	1 24	24 38 07		

(Evening column, printed vertically:) Owing to the shortness of the days, evening observations discontinued.

In taking the mean, the morning observation on the 12th *is rejected*, being unusually great, for which there was no apparent cause.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Feb.		Inches.				Feet.		
	Morn ...	29·518	36°	85°	SSE		Fog	35•
1	Noon....	29·447	42	64	S		Very fine	43
	Even	—	—	—	—		—	28
	Morn....	29·350	31	80	E by S		Cloudy	
2	Noon....	29·350	35	68	NE by E		Cloudy	35
	Even	—	—	—	—		—	31
	Morn....	29·482	31	77	NW		Cloudy	
3	Noon....	29·498	34	70	NW by N		Cloudy	34
	Even	—	—	—	—		—	30
	Morn....	29·560	31	73	W by N		Cloudy	
4	Noon....	29·560	35	67	SSW		Cloudy	36
	Even	—	—	—	—		—	30½
	Morn....	29·475	36	87	SSE		Rain	
5	Noon....	29·451	40	87	SSW		Fog, rain	41
	Even	—	—	—	—		—	35
	Morn....	29·343	40	87	SE by E		Foggy	
6	Noon....	29·440	44	71	W		Cloudy	46
	Even	—	—	—	—		—	39⅔
	Morn....	29·664	45	95	SSW	...	Foggy	
7	Noon....	29·678	49	89	SW	...	Cloudy	49
	Even	—	—	—	—		—	42
	Morn....	29·659	43	91	SW		Sm. rain	
8	Noon....	29·675	44	89	SSW	...	Cloudy	45
	Even	—	—	—	—		—	40
	Morn....	29·579	40	92	S by W	...	Foggy	
9	Noon ...	29·480	50	68	SSW		Very fine	50
	Even	—	—	—	—		—	40
	Morn....	29·378	43	88	W	...	Rain	
10	Noon....	29·470	45	70	NW		Fine	46½
	Even	—	—	—	—		—	31⅔
	Morn....	29·640	34	87	SW		Hazy	
11	Noon....	29·593	45	82	SSW		Cloudy	45½
	Even	—	—	—	—		—	34
	Morn....	29·392	37	87	NNW		Foggy	
12	Noon....	29·443	41	83	ENE		Rain	42
	Even	—	—	—	—		—	37
	Morn....	29·573	38	83	SSE		Cloudy	
13	Noon....	29·550	40	75	W by S		Cloudy	41
	Even	—	—	—	—		—	35
	Morn....	29·725	34	79	NNE		Cloudy	
14	Noon....	29·752	40	60	NNW		Very fine	42
	Even	—	—	—	—		—	29
	Morn....	29·827	31	87	ENE		Fog	
15	Noon....	29·838	34	80	ENE		Hazy	34
	Even	—	—	—	—		—	24
	Morn....	29·783	24	84	NE		Foggy	
16	Noon....	29·735	34	75	SW		Fine	34½
	Even ...	—	—	—	—		—	24
	Morn....	29·600	27	74	ESE		Clear	
17	Noon....	29·595	32	64	ESE		Fine	32
	Even	—	—	—	—		—	22
	Morn....	29·590	26	73	ENE		Clear	
18	Noon....	29·590	32	65	NE		Cloudy	32½
	Even	—	—	—	—		—	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Feb.		Inches.				Feet.		
19	Morn....	29·625	30°	71°	NE		Cloudy	28
	Noon....	29·630	33	70	NE		Cloudy	34
	Even	—	—	—	—		—	} 27
20	Morn....	29·379	—	85	NW		Snow	
	Noon....	29·363	—	80	Var.		Snow	—
	Even	—	—	—	—		—	} 26½
21	Morn....	29·357	30	85	S		Fog	
	Noon....	29·368	32	74	NE		Fog	34
	Even	—	—	—	—		—	} 32
22	Morn....	29·344	33	96	NE		Fog, rain	
	Noon....	29·340	40	90	E		Foggy	45
	Even	—	—	—	—		—	} 38
23	Morn....	29·129	45	93	SSE		Rain	
	Noon....	29·100	46	85	SSE		Cloudy	47
	Even ...	—	—	—	—		—	} 36
24	Morn....	28·984	36	89	W by S		Cloudy	
	Noon....	28·902	38	85	W by N		Rain	38
	Even	—	—	—	—		—	} 32
25	Morn....	28·969	33	88	N		Snow	
	Noon....	29·008	37	83	NE		Showery	39
	Even	—	—	—	—		—	} 32
26	Morn....	29·450	33	86	NE by E		Stormy	
	Noon....	29·533	32	70	NE by E		Cloudy	33⅔
	Even....	—	—	—	—		—	} 27⅔
27	Morn....	29·608	29	77	E by N		Cloudy	
	Noon....	29·600	35	64	ENE		Very fine	36
	Even	—	—	—	—		—	} 25⅔
28	Morn....	29·528	28	76	NE		Very fine	
	Noon....	29·469	37	63	E by N		Very fine	38¼
	Even	—	—	—	—		—	} 24¼
29	Morn....	29·382	28	70	WNW		Clear	
	Noon....	29·316	38	64	W		Very fine	40
	Even	—	—	—	—		—	

Rain, by the pluviameter, between noon the 1st of February, and noon the 1st of March, 1·143 inch. Evaporation, during the same period, 0·765 inch.

Article XII.

METEOROLOGICAL TABLE.

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1820.	Wind.	BAROMETER. Max.	BAROMETER. Min.	THERMOMETER. Max.	THERMOMETER. Min.	Evap.	Rain.	Hygr. at 9 a. m.	
2d Mo.									
Feb. 1	S E	30·09	29·96	45	22			96	
2	E	30·05	29·95	46	27			97	
3	N E	30·08	30·05	36	30			93	
4	S W	30·15	29·08	40	27			80	
5	S E	30·08	30·97	45	38		—	92	
6	Var.	30·20	29·97	48	38			93	
7	W	30 22	30·20	51	38			99	D
8	S W	30·24	30·14	47	35			88	
9	S	30·14	29·95	53	35			99	
10	W	30·20	30·14	50	29		—	99	
11	N W	30·14	29·97	49	35		10	93	
12	W	30·10	29·97	43	38		05	95	
13	S E	30·29	30·09	46	27			75	
14	N	30·39	30·29	45	30			97	●
15	N E	30·42	30·07	38	24			91	
16	N W	30·37	30·24	38	18			73	
17	N E	30·24	30·21	35	15	55		83	
18	N E	30·22	30·18	52	23		—	87	
19	N E	30·22	30·00	36	23		—	93	
20	Var.	30·00	29·99	37	27		—	95	C
21	S W	29·99	29·95	43	29		60	100	
22	N W	29·95	29·72	50	34		03	100	
23	S E	29·72	29·58	53	37		—	96	
24	S W	29·58	29·49	42	34		13	97	
25	N E	30·02	29·51	41	33		08	95	
26	N E	30·21	30·02	36	29			91	
27	N E	30·21	30·13	38	26			71	
28	N E	30·13	30·01	40	20			78	
29	S W	30·01	29·62	42	24	35	02	99	☉
		30·42	29·49	53	15	0·90	1·01	100—71	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month.—1. Hoar frost: very fine day. 3—6. Cloudy. 7, 8. Fine. 9. *Cirrocumulus:* fine. 10. Foggy morning: drizzly. 11, 12. Cloudy: some rain. 13. Very fine morning. 14, 15. Fine. 16. Hoar frost: misty: then fine, with *Cirrocumulus.* 17, 18. Hoar frosts: clear, a. m. 19. Overcast: some snow in the evening. 20. Ground covered with snow this morning from two to three inches in depth: it continued to snow, with very little intermission through the day. 21. Foggy morning, with thaw: about five inches of snow on the ground. 22—24. Overcast. 25. Overcast: windy. 26. Bleak wind. 27. Fine. 28, 29. Hoar frost: fine, with *Cirrocumulus.*

RESULTS.

Winds: N, 1; NE, 9; NW, 3; W, 3; SW, 5; S, 1; SE, 4; E, 1; Var. 2.

Barometer: Mean height

        For the month. ....,........ 30·058 inches.
        For the lunar period, ending the 6th. .... 30·005
        For 15 days, ending the 3d (moon north) .... 29·937
        For 13 days, ending the 16th (moon south) .... 30·166

Thermometer: Mean height

        For the month.................................. 36·379°
        For the lunar period, ending as above .... 32·766
        For 29 days, the sun in Aquarius. .... 35·155

Hygrometer: Mean for the month 91

Evaporation... 0·90 inch.

Rain .... 1·01

Mean temperature at Tottenham .... 36·827°

Hygrometer at ditto... 81°

Rain at ditto .... 1·27 in.

*** The late winter may be considered as having ended with the deep snow on the 20th, followed by a thaw on the 21st, though the spring has been frosty at times since: this snow occurred just 60 days after the shortest day ; and the snow on the 21st of tenth month, 1819, was just 60 days before it: thus the winter may be said to have lasted 120 days, with some mild intervals, the solstice being in the midst of the time.

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ERRATUM IN LAST MONTH.
For *Aquarius* read *Capricorn.*

Laboratory, Stratford, *Third Month,* 25, 1820.   L. HOWARD.

ARTICLE I.

Biographical Account of Dr. James Bradley, F.R.S. Astro-nomer Royal.

PERHAPS as an observer Dr. Bradley has never been surpassed by any astronomer whatever; while his two grand discoveries of the aberration of the fixed stars and of the nutation of the earth's axis constitute a memorable era in astronomical science, and raise their author to the very highest rank among the cultivators and promoters of that most sublime and useful science. I am desirous on that account to insert a short biographical sketch of his life in the *Annals of Philosophy ;* though I have nothing whatever to add to the account published in the 62d volume of the Histoire de l'Academie Royale des Sciences, from which all the lives of Dr. Bradley, which have appeared in different English publications, have been translated with greater or less accuracy.

James Bradley was born at Sherborne, in Dorsetshire, in the year 1692, and was the third son of William and Jane Bradley. He received the early part of his education at North Leach, in a boarding school kept by Messrs. Egles and Brice, who are said to have used their best endeavours to cultivate the happy genius which they observed in their pupil. From this place he went to Oxford with the intention of studying theology, and of taking orders. As soon as he was of sufficient standing to take holy orders, the Bishop of Hereford, who had conceived a great esteem for him, gave him the living of Bridstow, and soon after he was *inducted into* that of Welfrie, in Pembrokeshire. But

notwithstanding these advantages, which seemed to promise him the likelihood of advancing to still higher dignities in the church, he was induced at length to resign both his livings that he might be at full liberty to indulge his passion for mathematics and astronomy. The voice of nature is all powerful, and often destroys at once those arrangements in which she had not been sufficiently consulted.

Mr. Bradley was the nephew of Mr. Pound, well known in the republic of letters for several excellent astronomical observations. Indeed he would probably have published a great deal more had not the journal of his travels been destroyed in the conflagration at Pulo-Condor. This conflagration accompanied the massacre made by the inhabitants of the island of all the English in the place. Indeed Mr. Pound himself very narrowly escaped the fate which attended so many of his countrymen. It was with this relation that Mr. Bradley spent all the time which he could spare from his livings. He seems to have acquired by his own industry, without any other teacher, a sufficient knowledge of the mathematical sciences to relish and profit by the conversation of his uncle.

It is easy to conceive that the example and the conversation of his uncle did not render the official duties of Mr. Bradley more agreeable. He continued, however, to discharge them with assiduity, though he cast at times a wishful eye upon the heavens, and began at that time to lay the foundation of those discoveries which have raised him to the ranks of the greatest astronomer of his time.

Though these observations were made in some measure by stealth, the name of Bradley became famous enough to reach the ears of the most illustrious names in England. Lord Macclesfield, Sir Isaac Newton, Dr. Halley, and various other celebrated members of the Royal Society, became known to him, and cultivated his friendship. It was the estimation in which he was held by these great men that led to his becoming a Fellow of the Royal Society.

About this time, Dr. Keil, who filled the place of Savilian Professor of Astronomy at Oxford, died. It would have been difficult to have found so good a successor as Mr. Bradley, whether we consider his abilities or his fondness for astronomy. He was unanimously elected to the chair on October 31, 1721. Thus at the age of 22, he found himself the colleague of the celebrated Dr. Halley, who was at that time Savilian Professor of Geometry in the same university. Dr. Bradley, as soon as he was inducted into this chair, resigned both his livings in the church. He had long felt the disagreeable situation in which he was placed, his official duties as a clergyman, and his passion for astronomy drawing him contrary ways. And he took the *first opportunity* that offered to put an end to his constraint.

He was now at liberty to indulge his passion for astronomy

without interruption; and in 1727 he enabled others to enjoy the fruit of his researches by publishing his theory of the aberration of the fixed stars, one of the most useful and ingenious discoveries of modern astronomy.

It had been long observed that the position of the stars undergoes certain variations which do not in the least correspond with the apparent motion of one degree in 72 years, which is produced by the precession of the equinoxes.

The Abbé Picard had remarked these variations in the pole star as early as 1671, but he had neither attempted to reduce them to a constant rule, nor to assign any cause for them. The very numerous observations of Dr. Bradley presented him not merely with the variations observed by Picard, but with many others which had not so much as been suspected. He met with some stars which appeared during the course of a year to change their longitude without any alteration in their latitude; others appeared to alter their latitude without any alteration in their longitude; while others, and this was the case with the greatest number, appeared to describe a small ellipse more or less elongated.

The annual period which all these movements, so different from each other, affected, soon led to the inference that the motion of the earth was intimately connected with the phenomena. But the difficulty was to explain in what way it could produce such effects. The first attempts of Dr. Bradley to obtain an explanation were unsuccessful. But his perseverance was at last crowned with success, and enabled him to discover that all these apparent motions in the stars were the result of the successive motion of light combined with that of the earth round the sun.

It had been long believed that the velocity of light was, physically speaking, infinite. M. Roemer was the first who ventured to affirm that this opinion was inaccurate, and even to assign the time which light takes to traverse the diameter of the earth's orbit. He had observed that the emersion of the first satellite of Jupiter became later and later in proportion as Jupiter became further and further removed from the opposition; and that this retardation in an eclipse the nearest possible to the conjunction amounted to 11 minutes. He was of opinion that these 11 minutes constituted the time that the first ray of the satellite, when it emerged, took in traversing the distance between the two positions of the earth, when near the opposition and near the conjunction; and consequently that the velocity of light is not merely finite, but measurable.

However reasonable this explanation is now esteemed, it was then thought too bold; and it was not till long after the death of Roemer that astronomers unanimously agreed that the motion of light was successive. It was from this successive motion that *Dr. Bradley* obtained the explanation of the irregular varia-

tions which he had observed in the stars, and to which he gave the name of the *aberration of the fixed stars.* Let us now endeavour to communicate to the reader an idea of his explanation.

Let us conceive piles of small bodies moving in directions parallel to each other ; as, for example, a rain without any wind, falling down perpendicularly. Let us expose to this rain an immoveable tube placed in the same vertical position. It is obvious that the drop which enters at the upper orifice of the tube will pass out at the other end without touching the inner walls of the tube.

But if we make the tube move parallel to itself, though its position always remains parallel to the direction of the drops of rain, it is obvious that the motion of the tube will cause the drops to strike against one of its sides, and that the sooner, according as the motion of the drops is slower, compared with that of the tube. And it is easy to demonstrate that if the motion of both be equal, the drop of rain which falls upon the centre of the upper opening of the tube will strike the inside wall after having traversed exactly half the semidiameter of the tube ; and that its direction in consequence will make an angle of 45° with the axis of the tube. Hence it follows that if we wish the drops of water not to touch the tube notwithstanding its motion, we must incline it 45° in the direction of its motion. If this motion were to take place in the circumference of a circle, the tube would describe round the vertical line passing through the centre of its base, a cone, the angle of which will be 90°.

What has been said is meant to show that the inclination of the tube in order to allow the drops of rain, notwithstanding the motion, to pass through the tube without striking against its sides, depends entirely upon the proportion between the velocity of its motion and that of the drops of rain. The greater the velocity of these drops compared with that of the tube, the less will it be necessary to incline the tube. Hence if the velocity of the drops were infinite compared with that of the tube, it would not be necessary to incline the tube at all ; because the drop would reach the bottom of the tube the very instant that it entered its top, and the tube, during such a space of time, could advance only an infinitely small quantity.

When we apply this theory to the aberration of the stars it is easy to see that the lines traversed by the drops of rain are the rays proceeding from the stars ; that the tube which we have supposed at first at rest, and afterwards in motion, is that of the telescope, which serves to determine the position of the stars, and which is always carried away by the motion of the earth round the sun; and finally, that the velocity of light being finite, *when compared* with that of the earth in its orbit, the telescope *must change* its position in proportion as this motion changes *its direction.* Hence it follows that each star must have a series

of different positions, or, which comes to the same thing, an apparent motion in the heavens, which will make it describe in a year ellipses more or less elongated according to the position of the star.

Such is the theory of the aberration of light which Dr. Bradley published in 1727, and which was received by astronomers with that applause which it deserved. M. Clairaut made it the subject of an excellent memoir printed in the Memoirs of the French Academy of Sciences for 1737, in which he examined the theory of aberration to the bottom, and gave the rules necessary to apply it to practice. The result of his calculation is, that the velocity of light deduced from the aberrations observed in the stars is absolutely the same as that assigned it by the ingenious explanation which Roemer had given of the retardation of the eclipses of the first satellite of Jupiter. This is a new proof of the accuracy of the hypothesis, if it stood in need of being proved.

Three years after this glorious epoch in the life of Dr. Bradley, the place of Reader in Astronomy and Physics in the Museum at Oxford became vacant. It was bestowed upon him, and certainly no individual in the university was better qualified for the task thus assigned him.

Dr. Bradley's diligence as an observer was redoubled by the increase of his reputation. He gradually discovered that the inclination of the axis of the earth on the plane of the ecliptic was not constant; but underwent a variation amounting to some seconds, the period of which was nine years. This period seemed at first to bid defiance to all explanation. What could a period of nine years have in common with the revolution of the earth round the sun which is completed in one year? Dr. Bradley was, however, fortunate enough to find the true cause in the Newtonian theory of attraction.

The first principle of this theory, it is well known, is, that all bodies attract each other mutually directly as their mass, and inversely as the square of their distances. From this attraction combined with rectilineal motion, Newton deduced the orbits of the planets, and, in particular, the orbit of the earth. If that orbit were a circle, and if the globe of the earth were exactly spherical, the attraction of the sun would act only to keep it in its orbit, and would not derange the position of its axis. But neither of these suppositions is true. The earth is sensibly greatest at the equator, and its orbit is an ellipse in one of the foci of which the sun is placed. When the position of the earth is such that the plane of its equator passes through the centre of the sun, then the sun has no other action but that of drawing the globe towards itself; but always parallel to itself, and without deranging the position of its axis. This happens at the two equinoxes. As the earth recedes from these two points, the sun leaves the *plane of the* equator, and approaches to one or other

of the tropics, then the two semidiameters of the earth exposed to the sun, being no longer equal, the equator is more strongly attracted than the rest of the globe, which alters a little its position and its inclination to the plane of the ecliptic. And as the part of the orbit, included between the autumnal and vernal equinox is smaller than that included between the vernal and the autumnal equinox, it follows that the derangement caused by the sun, while it passes through the northern signs, is not entirely compensated by that produced while it passes through the southern signs; and that the parallelism of the terrestrial axis and its inclination with the ecliptic remain a little altered. Hitherto we see nothing which has any relation to the period of nine years. We shall see immediately what produces this period.

What the sun operates upon the earth by its attraction, the moon operates also, and it acts with the greater effect the more it deviates from the equator. But when its nodes concur with the equinoxial points, its greatest latitude is added to the greatest obliquity of the ecliptic. This then is the time of its greatest action to derange the position of the terrestrial axis. And the revolution of the nodes of the moon occupying a period of 18 years, it is clear, that during that period the nodes will be twice in the equinoxial points; consequently, during that period, the axis of the earth will be the most deranged possible two several times. Thus the axis will be the most deranged possible once every nine years ; or, which comes to the same thing, it will have a vibration, the period of which will be nine years, as Dr. Bradley had observed. This vibration is what he termed the *nutation of the terrestrial axis*. He published an account of it in 1737. Thus within 10 years he communicated to the public two of the greatest discoveries in modern astronomy—discoveries which will always mark a memorable epocha in the history of the science.

Dr. Bradley had always enjoyed the esteem and the friendship of Dr. Halley, at that time Astronomer Royal, but in a very advanced period of life, and unable to contribute as usual to the promotion of his favourite science. He conceived that he could not confer a greater favour on it than by endeavouring to procure Dr. Bradley for his successor. With this view he wrote to Dr. Bradley several letters, which were found among that gentleman's papers after his death, requesting permission to solicit for him the reversion of his office, and even offering, if necessary, to resign in his favour. But Dr. Halley died before he was able to accomplish this desirable object. The Earl of Macclesfield, however, well known for his attachment to astronomy, and afterwards President of the Royal Society, had sufficient interest to secure him the situation of Astronomer Royal. As soon as the nomination was publicly known, the University of Oxford *enrolled him* as one of their own body by creating him Doctor *in Divinity*.

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The situation of Astronomer Royal was the real element of Dr. Bradley. He devoted himself to observations with the most indefatigable industry; so that the remainder of his life constitutes, so to speak, a portion of the history of the heavens.

Though the collection of instruments at Greenwich was already very considerable, it was impossible that so ardent an observer as Dr. Bradley could avoid wishing for various others, both to ensure a greater degree of precision, and to suit his own particular views. At the annual visit of the committee of the Royal Society in 1748 he laid an inventory of the apparatus before that learned body, and represented in such strong terms, the necessity of getting the old instruments repaired and new ones constructed, that the Society deemed it requisite to lay the representation before the king, who was pleased to grant 1000*l.* for the purposes pointed out by the Astronomer Royal. Messrs. Graham and Bird were immediately set to work, and the observatory was soon provided with the most complete set of apparatus, which the state of the arts at that time admitted. The observations made by Dr. Bradley were exceedingly numerous; and it may be said with truth that they form the first collection of rigidly accurate astronomical observations ever presented to the public. They constitute a kind of epocha in astronomy, by rendering it necessary for all subsequent observers to provide themselves with instruments of the requisite delicacy and precision, and of taking the necessary pains to ensure the accuracy of their observations.

Soon after his going to Greenwich to reside, the Rectorship of that parish became vacant, and it was offered to Dr. Bradley; but he was disinterested enough to decline the offer, fearing that his duties as a clergyman and as an astronomer might interfere with each other. George II. was so much struck with this disinterested refusal, that he gave him a pension of 250*l.* a year in the beginning of 1752. The reason assigned was his uncommon skill in astronomy and in other parts of the mathematics, and the advantages resulting to the commerce and navigation of Great Britain from the application which he made of that skill.

In the year 1747 Dr. Bradley was chosen a member of the Royal Academy of Sciences of Berlin: the year after, he was made Foreign Associate of the Academy of Sciences of Paris: in 1754 he became a member of the Imperial Academy of St. Petersburgh: and in 1757 of the Institute of Bologna.

He continued his unremitting attention to the duties of his situation till towards the end of 1760, when he was seized with a malady that deprived him of his strength. For two whole years he experienced no other inconvenience; but about the end of June, 1762, he was seized with a suppression of urine in consequence of an inflammation of the kidneys. Of this disease he died on July 13, in the 70th year of his age. He was buried at Michin-*Hampton*, in Gloucestershire, in the same place where

his mother and his wife had been already interred; for in 1744 he had married Susanna Peach, the daughter of a gentleman in Gloucestershire, by whom he had a daughter, who survived her father.

The most striking part of his character was the most perfect modesty and a sweetness of temper very uncommon in a man of his lively temperament, and capable of enduring the late nights and the intense application which occupied the whole of his life. His generosity was without bounds to those who required his assistance, and he was perfectly destitute of that selfishness with which literary men are so often reproached. Though he spoke well, and possessed the power of communicating his ideas with the most perfect clearness, he was remarkably silent, never intruding his opinion, except when it was necessary so to do. But when he thought that his conversation could be useful, he was not sparing of it. He even induced his disciples to put questions to him by the accuracy with which he answered them, and by the attention which he always paid to bring himself down to the level of those with whom he conversed. He was not more inclined to protrude his writings than his conversation upon the world. The consequence was that he published very little. He was so diffident of himself that he never was satisfied with his own compositions, and was induced to suppress a great many which probably were highly deserving of publication. Fortunately he was under the necessity as Astronomer Royal of communicating his observations to the Royal Society. The consequence was the preservation of the immense quantity which he had made.

He became celebrated almost in spite of himself. His merit alone, without any attempt on his part to attract attention, produced his reputation. In this respect he furnishes a striking contrast to some men of science of latter years, who have employed as much art and chicanery to attract the regard of the public, have caballed as much to detract from their supposed rivals; have made as great a sacrifice of truth and uprightness of conduct to secure to themselves a kind of monopoly of the particular science to which they had attached themselves, as if they thought themselves secure of blinding the whole of mankind, and of appropriating to themselves that exact share of reputation which they have thought proper to claim. Fortunately for the interests of science and of human nature this conduct has never in a single instance been ultimately successful. The cabals and the factions which have shut out the light from contemporaries gradually disappear, and when the leader of a scientific party is subjected to the lynx-eyed scrutiny of posterity, they never fail sooner or later of detecting all the false pretensions; of discovering the vanity, the selfishness, the malignity, *which our man* of science has displayed by his actions. The *consequence is,* that posterity not merely reduce him to the

exact place which he was entitled to occupy; but often place him much lower than the seat which his abilities and industry would have entitled him to occupy, had he been satisfied with that share of reputation which his real merit entitled him to obtain. The conduct of Bradley was exactly the opposite; yet few men enjoyed a higher reputation, or were more respected by all that was great and eminent in Great Britain or on the Continent during his own life time; and few men have retained, or are likely to retain, a higher place in the most exalted and most perfect of all the sciences.

ARTICLE II.

Regular Crystallization of Olive Oil. By Dr. Clarke, Professor of Mineralogy, Cambridge.

(To Dr. Thomson.)

DEAR SIR,   *Cambridge, March* 22, 1820.

An accident has occurred within the last week which has enabled me distinctly to observe the regular crystallization of *olive oil.* The thermometer of *Fahrenheit*, during the late north winds, has frequently indicated a temperature below 40° at noon-day. Upon one of these occasions, when the mercury had fallen, towards evening, to 35°, some sallad oil, instead of exhibiting the usual appearance, by congelation, of having lost its transparency, presented to the eye a number of white, opaque, prismatic radii, rising upwards from the bottom of the vessel, and beautifully diverging in the transparent fluid. When examined with a lens, these prisms (which were as large as the capillary prisms of radiating *arragonite* in porous *trap*) were found to have the form of *mesotype*; that is to say, rectangular four-sided prisms with square bases. The terminating planes, being squares, reflected the light in such a manner as to enable me to discern their form in the most satisfactory manner. Several persons witnessed this appearance; and among others our geological Professor, *Sedgewick*, and Mr. *Henslow*, of St. John's College. I endeavoured to preserve the oil in this state, but the weather becoming warmer, the whole of the fluid became transparent. Its crystalline appearance was, however, preserved during 24 hours; and often, during this time, examined. The inference I would deduce from this circumstance is, that the crystallization of this *vegetable oil* agrees with the general phenomena of crystallization characteristic of non-metallic combustibles; among which the *octahedron*, whether regular, as in *diamond;* or obtuse, as in *mellite;* or acute, as in *sulphur;* exhibits pyramids whose bases are squares.

E. D. CLARKE.

ARTICLE III.

Extract of a Letter from Mr. Breithaupt, in Freiberg, to Professor Gilbert. [*]

You are aware that different chemists have found boracic acid in the tourmalin, axinite, &c. of which essential constituent the most celebrated former analyses have not taken the least notice. These researches were undertaken at my request. I have been employed these five years in endeavouring to construct a mineral system, which should indeed depend entirely upon the natural characters and properties of minerals; but I wish it to be at the same time chemical, physical, and philosophical. Though it may, and indeed must appear to you a piece of bombast to affirm that it is the first system ever contrived, the assertion is true notwithstanding. I do not choose to publish an account of it till the whole has been put in better order, though it is true at the same time that a considerable part of it has become already, in some measure, known to the public.

My fundamental maxim is, that *individual minerals* (and only crystals are individuals) *owe their natural characters to their chemical constituents.* Hence the shape of the crystals, the lustre, the electricity, the degree of hardness, the specific gravity, &c. must depend upon the nature of the constituents. But if these conclusions be true, it is obvious that little dependence can be put upon the chemical analyses of minerals, which we already possess. I have, for example, put boracite, tourmaline, anatase, andalusite (with respect to it I have been unsuccessful in my attempts to discover an essential ingredient), and axinite, under one family. And as the external characters of these minerals bear a close resemblance to each other, I expect from my theoretic maxim, to which I have given the name of *plasticism*, to find an equal correspondence in the constituents. I, therefore, conclude, that as *boron is an essential constituent of boracite, the same substance must also be an essential constituent of tourmaline, anatase, andalusite, and axinite.* The proof that boron is essential to the boracite may be deduced from the principles of crystallography.

Lampadius has already found about 16 per cent. of boracic acid in the tourmaline and axinite; and I now learn from my friend Christian Gmelin that he himself, Berzelius, Arfvedson, and Vogel, have made experiments which confirm this result.

I have a treatise upon the family of schorls quite ready, and I shall make it known as soon as I can procure a good draughts-

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[*] Translated from Gilbert's Annalen, lx. 211.

man to delineate 13 varieties of crystals. I shall state some of it here.

The essential constituents of the schorl family are not confined to boron. They are

1. Electropositive.

a. Either an *alkaline earth* or *alkali.* Among these the oxide of zinc is to be ranked; perhaps even oxide of iron or manganese may be substituted; or all of these may exist together.

b. Alumina. (It is essential even in boracite, which probably contains some per cents. more than is at present known.) It has still to be pointed out in anatase.

2. Electronegative.

a. Silica, or rather quartz, for which oxide of titanium is sometimes substituted.

b. Boron. (The appearances which the schorl family exhibit when thrown into red-hot saltpetre, demonstrate that, *boracite, &c.* contains a combustible substance (*boron*), not a product of combustion (*boracic acid*); although this last is the substance obtained by analysis. It is a product, not an educt. Boron is still to be found in anatase and andalusite.

The substances upon which the system of crystallization, and consequently the species depend, are

1. In boracite (tetrahedral schorl) magnesia. Boracite is therefore a magnesian schorl.

2. In tourmaline (trigonal schorl) the quartz substance. It is therefore siliceous, or quartz schorl.

3. In anatase (tetragonal schorl) titanium. It is therefore a titanium schorl.

4. In andalusite (rhombic schorl) a substance not yet discovered experimentally; very probably fluorine.

5. In axinite (rhomboidal schorl) boron. (This substance therefore, is not merely essential as a generic, but still more essential as a specific substance; and I first ascertained the necessity of its presence in this mineral from theory.) It is consequently a boronic schorl.

Boron is indeed an essential constituent of *datholite;* but as in it the hardness of schorl is wanting; so likewise is the constituent which gives hardness; namely alumina.

You perceive that I have not reposed upon any authority. In all systematic disputes in the dominions of nature, the sole and ultimate arbiter is nature herself. And in the mineral kingdom nature has expressed here classes and orders by the external characters. In the schorls we perceive the same lustre, the same colours, the same play of light, the same polarity of crystallization (determined in my treatise), the same hardness, the same specific gravity, from 2·9 to 3·3 (and if that of anatase be correct, as high as 3·8), the same electricity, &c. Hence it is evident how completely nature has given us the means of classi-

fying mineral bodies according to their characters. It is true that boracite has been arranged among the salts, like a fish with stag's horns. Although its quartzy hardness, long ago well known to Ilsemann, showed clearly that it belonged to the precious stones.

I presume likewise that boron exists in helvine, dioptase, sphene, cyanite, tetrahedral fahlore, and even in Rothgilti- gerze. I am almost certain that boron is a constituent of wolfram.

All this is nothing to the number (above 40 species, according to Werner's system) of minerals in which theory has pointed out to me the existence of *chlorine* as an essential constituent some- times generically, sometimes specifically, though analytical chemistry has not been able hitherto to verify these theoretical conclusions. Thus it exists in schaumkalk, talc, mica, schiller- stone, felspar, cyanate, &c. and in many uncrystallized minerals, as steatite, serpentine, nephrite, chalk, mountain soap, fuller's earth, &c. One will not easily miss chlorine in a fatty, soft mineral, and the talky properties are produced not less by it than by magnesia.

As chlorine and iodine resemble each other very closely, but the latter has more of the metallic properties, I presume that both, but more probably the latter, exists in graphite, molybdena, blättererz, black earthy cobalt, manganschaum, &c. A person may easily satisfy himself that the essential constituent of graphite is not yet known, and that a substance exists in it which has not yet been shown experimentally to constitute a part of the mineral kingdom.

But I must be shorter than I have been. I write at present on purpose to obtain the assistance of chemists. Already has chemistry been of the greatest service to mineralogy; and it is now time that this last science should atone for her faults towards her benefactor. I hope within a year to publish a System of Mineralogy which will not merely consider crystals and crystallized species as belonging to pure natural history, but with respect to which chemists will have no reason to complain of intolerance.   AUGUST BREITHAUPT.

Freiberg, Aug. 6, 1818.

ARTICLE IV.

Meteorological Results of the Atmospherical Pressure and Temperature, Rain, and Wind, deduced from diurnal Observations, made at the Apartments of the Royal Geological Society of Cornwall, Penzance, in 1819. By E. C. Giddy, Esq. Surgeon, Curator of the Cabinet. (Communicated by the Secretary, Dr. Forbes.)

1819. Months.	BAROMETRICAL PRESSURE.*					TEMPERATURE.					WEATHER.			WINDS.										
	Mean.	Maximum.	Minimum.	Range.	Variation in 24 hours.	Mean at both observations.	At two, p.m.	At eight, a.m.	Range.	Variation in 24 hours.	Rain in inches.	Wet days.	Dry days.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Prevailing winds.	Brisk winds.	Boisterous winds.
Jan.......	29·480	30·153	28·768	1·385	0·530	46·0	56	37	19	8	3·24	25	6	1	2	1	3	6	3	5	6	NW	13	6
F·b.......	29·470	29·874	28·854	1·020	0·472	46·0	56	38	18	7	3·02	22	6	4	2	0	1	2	5	9	5	W	13	3
March...	29·681	30·021	28·879	1·142	0·362	48·0	60	36	24	6	0·95	10	21	1	4	0	1	1	4	7	4	E	8	1
April.....	29·507	29·941	28·942	0·999	0·381	53·0	66	44	22	7	1·24	13	17	6	1	4	4	1	4	5	2	N	8	0
May......	29·609	29·905	29·289	0·616	0·189	58·0	70	48	22	6	2·09	16	15	3	3	4	2	4	4	4	2	SE	8	1
June......	29·661	30·003	29·203	0·800	0·295	61·0	70	55	15	8	2·00	19	11	10	1	2	12	1	4	4	5	N	4	0
July......	29·728	29·937	29·100	0·837	0·491	65·5	78	56	22	8	0·21	4	27	8	5	0	2	0	7	9	5	N	4	1
Aug.......	29·694	29·926	28·945	0·981	0·319	67·5	78	56	22	6	1·32	7	24	3	4	2	9	1	1	6	5	SE	4	0
Sept.......	29·669	30·092	29·262	0·830	0·289	61·0	70	48	22	5	1·55	13	17	3	3	2	7	1	8	1	4	SW	6	0
Oct.......	29·611	30·071	29·138	0·933	0·412	53·5	66	38	28	8	1·98	13	18	7	2	3	2	1	4	2	2	N,W	7	2
Nov.......	29·525	29·855	29·014	0·841	0·397	46·0	56	32	24	14	2·36	19	11	5	2	5	1	4	3	5	8	N,W	7	2
Dec.......	29·488	29·940	28·958	0·982	0·648	42·0	56	26	30	11	3·76	20	11	2	6	2	2	0	6	4	9	NW	16	0
Annual Means, &c.	29·593	30·153	28·854	11·366	0·648	54·0	65	43	268	8	23·72	181	184	53	35	30	45	22	55	65	58	W	92	16

* The height of the column of mercury in these tables is reduced to what it would have been had the attached thermometer stood at 32°.

ARTICLE V.

Tabular View of a Meteorological Journal kept at Lancaster. By Mr. J. Heaton.

1819. Months	THERMOMETER				BAROMETER			WEATHER		WINDS							
	Highest	Lowest	Mean	Mean diurnal variation	Highest	Lowest	Mean	Wet days	Fair days	S.	S.W.	W.	N.W.	N.	N.E.	E.	S.E.
Jan	50°	31°	39.5	4.9	30.48 (1)	28.84 (25)	29.64	19	12	12	7	4	0	1	2	2	3
Feb	48	30	39.7	5.9	30.05 (14)	28.92 (21)	29.55	20	8	3	10	2	3	3	1	1	5
March	52	31	43.3	8.0	30.29 (14)	29.31 (19)	29.88	11	20	1	14	3	4	3	5	0	1
April	60	37	47.7	10.9	30.22 (26)	29.22 (17)	29.79	9	21	0	4	7	3	1	1	9	5
May	69	38	54.3	11.5	30.16 (14)	29.60 (4)	29.92	13	18	2	5	9	2	0	2	7	4
June	71	48	57.4	11.3	30.27 (20)	29.36 (7)	29.87	16	14	1	13	9	6	0	1	0	0
July	77	48	62.1	13.3	30.28 (28)	29.36 (19)	30.01	10	21	4	3	15	4	1	0	3	1
August	80	51	64.8	13.4	30.40 (18)	29.01 (31)	30.02	7	24	3	3	12	4	1	2	1	5
Sept	71	40	55.9	11.0	30.53 (21)	29.35 (1)	29.93	11	19	6	10	5	0	5	2	2	0
October	67	27	49.0	10.0	30.37 (15)	29.34 (23)	29.83	14	17	5	8	2	4	4	7	1	0
Nov	54	24	39.4	8.8	30.20 (18)	29.05 (6)	29.70	11	19	5	3	0	6	6	7	2	1
Dec	51	22	36.2	8.4	30.20 (8)	29.06 (18)	29.68	10	21	4	3	3	3	1	6	6	5
	Aug. 80	Dec. 22	49.11	9.8	Sept. 30.53	Jan. 28.84	29.818	151	214	46	83	71	39	26	36	34	30

ARTICLE VI.

Demonstrations of Theorems for finding the Sums of Sines.
By Mr. James Adams.

(To Dr. Thomson.)

SIR,   *Stonehouse, near Plymouth, Feb.* 8, 1820.

NEVER having seen any demonstrations of the theorems for finding the sums of the sines sin. a + sin. (a + b) + sin. (a + 2 b) + ... sin. (a + n b) and cos. a + cos. (a + b) + cos. (a + 2 b) + ... cos. (a + n b), I, therefore, beg leave to send you the following demonstrations thereof, the insertion of which in the *Annals of Philosophy* will oblige,

Your humble servant,

JAMES ADAMS.

Prob. 1.—To find the sum of sin. a + sin. 2 a + sin. 3 a + ... sin. n a.

Per Mr. Woodhouse's Trigonometry, p. 54,

$$\sin. a = \frac{1}{2\sqrt{-1}}\left(x - \frac{1}{x}\right)$$

$$\sin. 2 a = \frac{1}{2\sqrt{-1}}\left(x^2 - \frac{1}{x^2}\right)$$

$$\sin. 3 a = \frac{1}{2\sqrt{-1}}\left(x^3 - \frac{1}{x^3}\right)$$

$$\dots\dots\dots\dots\dots\dots\dots$$

$$\sin. n a = \frac{1}{2\sqrt{-1}}\left(x^n - \frac{1}{x^n}\right)$$

Then per geometrical progression,

$$x + x^2 + x^3 + x^4 + \dots x^n = \frac{x^{n+1} - x}{x-1}, \text{ and } \frac{1}{x} + \frac{1}{x^2} + \frac{1}{x^3} + \dots \frac{1}{x^n} = \frac{x^n - 1}{x^n(x-1)},$$

we then have sin. a + sin. 2 a + sin. 3 a + ... sin. n a =

$$\frac{1}{2\sqrt{-1}}\left(\frac{x^{n+1} - x}{x-1} - \frac{x^n - 1}{x^n(x-1)}\right) = \frac{1}{2\sqrt{-1}}$$

$$\frac{(x^{n+1} - 1)(x_n - 1)}{x_n(x-1)} = \frac{\left(x^{\frac{n+1}{2}} - \frac{1}{x^{\frac{n+1}{2}}}\right)\left(x^{\frac{n}{2}} - \frac{1}{x^{\frac{n}{2}}}\right)}{x^{\frac{1}{2}} - \frac{1}{x^{\frac{1}{2}}}} \times \frac{1}{2\sqrt{-1}} =$$

$$\frac{(2\sqrt{-1} . \sin. \frac{1}{2} n a)(2\sqrt{-1} . \sin. \frac{n+1}{2} a)}{2\sqrt{-1} \times \sin. \frac{1}{2} a} \times \frac{1}{2\sqrt{-1}} =$$

$$\frac{\sin. \frac{1}{2} n a \times \sin. \frac{n+1}{2} a}{\sin. \frac{1}{2} a}.$$

Corollary.—Sin. a + sin. $2\,a$ + sin. $3\,a$ &c. ad infinitum = $\frac{1}{2}$ cot. $\frac{1}{2}\,a$.

Per trigonometry, sin. $\frac{1}{2}\,n\,a$ × sin. $(\frac{1}{2}\,a + \frac{1}{2}\,n\,a) = \frac{1}{2}$ cos. $\frac{1}{2}\,a - \frac{1}{2}$ cos. $(\frac{1}{2}\,a + n\,a)$

But $n\,a$ being infinitely great, $\frac{1}{2}\,a + n\,a$ will likewise be infinitely great, and as the cosine of an infinitely great arc *may be* nothing, we shall have sin. a + sin. $2\,a$ + sin. $3\,a$, &c. ad infi-

nitum = $\dfrac{\frac{1}{2}\cos. \frac{1}{2}\,a}{\sin. \frac{1}{2}\,a} = \frac{1}{2}$ cot. $\frac{1}{2}\,a$.

Prob. 2.—To find the sum of cos. a + cos. $2\,a$ + cos. $3\,a$ + ... cos. $n\,a$.

Per Mr. Woodhouse's Trigonometry, p. 5,

$$\cos.\ a = \frac{1}{2}\left(x + \frac{1}{x}\right) \therefore 2\cos. a = x + \frac{1}{x}$$

$$\cos. 2\,a = \frac{1}{2}\left(x^2 + \frac{1}{x^2}\right) \therefore 2\cos. 2\,a = x^2 + \frac{1}{x^2}$$

$$\cos. 3\,a = \frac{1}{2}\left(x^3 + \frac{1}{x^3}\right) \therefore 2\cos. 3\,a = x^3 + \frac{1}{x^3}$$

$$\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots$$

$$\cos. n\,a = \frac{1}{2}\left(x^n + \frac{1}{x^n}\right) \therefore 2\cos. n\,a = x^n + \frac{1}{x^n}$$

Then by geometrical progression, we have

$$x + x^2 + x^3 + x^4 + \cdots x^n = \frac{x^{n+1} - x}{x - 1}, \text{and} \frac{1}{x} + \frac{1}{x^2} + \frac{1}{x^3} +$$

$$\cdots \frac{1}{x^n} = \frac{x^n - 1}{x^n\,(x - 1)}.$$

Therefore cos. a + cos. $2\,a$ + cos. $3\,a$ + ... cos. $n\,a$ = $\frac{1}{2}$

$$\left(\frac{x^{n+1} - x}{x - 1} + \frac{x^n - 1}{x^n\,(x - 1)}\right) = \frac{1}{2}\frac{(x^{n+1} + 1)\,(x^n - 1)}{x^n\,(x - 1)} = \frac{1}{2}$$

$$\left\{\frac{\left(x^{\frac{n+1}{2}} + \frac{1}{x^{\frac{n+1}{2}}}\right)\left(x^{\frac{n}{2}} - \frac{1}{x^{\frac{n}{2}}}\right)}{x^{\frac{1}{2}} - \frac{1}{x^{\frac{1}{2}}}}\right\} = \frac{\sin. \frac{1}{2}\,n\,a \times \cos. \frac{n+1}{2}\,a}{\sin. \frac{1}{2}\,a}.$$

Corollary.—Cos. a + cos. $2\,a$ + cos. $3\,a$, &c. ad infinitum = $-\frac{1}{2}$.

Per trigonometry, sin. $\frac{1}{2}\,n\,a$ × cos. $\left(\frac{1}{2}\,a + \frac{1}{n}\,a\right)$ = $\frac{1}{2}$ sin. $\left(\frac{1}{2}\,a + n\,a\right) - \frac{1}{2}$ sin. $\frac{1}{2}\,a$, but $n\,a$ being infinitely great, $\frac{1}{2}\,a + n\,a$ will likewise be infinitely great; and as the sine of an infinitely great arc *may be* nothing, we shall have cos. a + cos. $2\,a$ + cos. $3\,a$, &c. ad infinitum = $\dfrac{-\frac{1}{2}\sin. \frac{1}{2}\,a}{\sin. \frac{1}{2}\,a} = -\frac{1}{2}$.

Prob. 3.—To find the sum of sin. a + sin. $(a + b)$ + sin. $(a + 2\,b)$ + ... sin. $(a + n\,b)$.

Per trig. sin. $a =$ sin. a

$$\text{sin.}\,(a + b) = \text{sin.}\,a\,\text{cos.}\,b + \text{sin.}\,b\,\text{cos.}\,a$$
$$\text{sin.}\,(a + 2\,b) = \text{sin.}\,a\,\text{cos.}\,2\,b + \text{sin.}\,2\,b\,\text{cos.}\,a$$
$$\text{sin.}\,(a + 3\,b) = \text{sin.}\,a\,\text{cos.}\,3\,b + \text{sin.}\,3\,b\,\text{cos.}\,a$$
$$\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots$$
$$\text{sin.}\,(a + n\,b) = \text{sin.}\,a\,\text{cos.}\,n\,b + \text{sin.}\,n\,b\,\text{cos.}\,a$$

Then by problems the first and second, we have the sum of the series $=$

$$\frac{\text{sin.}\,a \times \text{sin.}\,\frac{1}{2}b + \text{sin.}\,\frac{1}{2}nb \times \text{sin.}\,a \times \text{cos.}\,\frac{n+1}{2}b + \text{sin.}\,\frac{1}{2}nb \times \text{cos.}\,a \times \text{sin.}\,\frac{n+1}{2}b}{\text{sin.}\,\frac{1}{2}b}$$

$$= \frac{\text{sin.}\,a \times \text{sin.}\,\frac{1}{2}b + \text{sin.}\,(a + \frac{1}{2}b + \frac{1}{2}nb) \times \text{sin.}\,\frac{1}{2}nb}{\text{sin.}\,\frac{1}{2}b} =$$

$$\frac{\text{sin.}\,(a + \frac{1}{2}nb) \times \text{sin.}\,\frac{1}{2}(n + 1)b}{\text{sin.}\,\frac{1}{2}b}. \quad \text{(I.)}$$

For sin. $a \times$ sin. $\frac{1}{2}b = \frac{1}{2}$ cos. $(a - \frac{1}{2}b) - \frac{1}{2}$ cos. $(a + \frac{1}{2}b)$
And sin. $(a + \frac{1}{2}b + \frac{1}{2}nb)$ sin. $\frac{1}{2}nb = \frac{1}{2}$ cos. $(a + \frac{1}{2}nb)$ $- \frac{1}{2}$ cos. $(a + \frac{1}{2}b + nb)$

Therefore, sin. $a \times$ sin. $\frac{1}{2}b +$ sin. $(a + \frac{1}{2}b + \frac{1}{4}nb)$ sin. $\frac{1}{4}nb$ $= \frac{1}{2}$ cos. $(a - \frac{1}{2}b) - \frac{1}{2}$ cos. $(a + \frac{1}{4}b + nb)$

But sin. $(a + \frac{1}{2}nb)$ sin. $(\frac{1}{2}b + \frac{1}{2}nb) = \frac{1}{2}$ cos. $(a - \frac{1}{2}b) -$ $\frac{1}{2}$ cos. $(a + \frac{1}{2}b + nb)$

Hence the equation marked (I.) is evident.

Corollary.—Sin. $a +$ sin. $(a + b) +$ sin. $(a + 2\,b) +$ sin. $(a + 3\,b)$ &c. ad infinitum $= \dfrac{\text{cos.}\,(a - \frac{1}{2}b)}{2\,\text{sin.}\,\frac{1}{2}b}$.

For in this case $n\,b$ is infinitely great; therefore $a + \frac{1}{2}b + n\,b$ is infinitely great; and since the cosine of an infinitely great arc *may be* zero, therefore the sum of the series $= \dfrac{\frac{1}{2}\,\text{cos.}\,(a - \frac{1}{2}b)}{\text{sin.}\,\frac{1}{2}b} = \dfrac{\text{cos.}\,(a - \frac{1}{2}b)}{2\,\text{sin.}\,\frac{1}{2}b}$.

Prob. 4.—To find the sum of cos. $a +$ cos. $(a + b) +$ cos. $(a + 2\,b) + \ldots$ cos. $(a + n\,b)$.

Per trig. cos. $a \ldots\ldots =$ cos. a

$$\text{cos.}\,(a + b) = \text{cos.}\,a \,.\,\text{cos.}\, b - \text{sin.}\,a\,.\,\text{sin.}\,b$$
$$\text{cos.}\,(a + 2\,b) = \text{cos.}\,a \,.\,\text{cos.}\,2\,b - \text{sin.}\,a\,.\,\text{sin.}\,2\,b$$
$$\text{cos.}\,(a + 3\,b) = \text{cos.}\,a \,.\,\text{cos.}\,3\,b - \text{sin.}\,a\,.\,\text{sin.}\,3\,b$$
$$\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots$$
$$\text{cos.}\,(a + n\,b) = \text{cos.}\,a \,.\,\text{cos.}\,n\,b - \text{sin.}\,a\,.\,\text{sin.}\,n\,b$$

Then by problems the first and second, the sum of the series $=$

$$\frac{\text{cos.}\,a \times \text{sin.}\,\frac{1}{2}b + \text{sin.}\,\frac{1}{2}nb \times \text{cos.}\,\frac{n+1}{2}b \times \text{cos.}\,a - \text{sin.}\,\frac{1}{2}nb \times \text{sin.}\,\frac{n+1}{2}b \times \text{sin.}\,a}{\text{sin.}\,\frac{1}{2}b}$$

$$= \frac{\text{cos.}\,a \times \text{sin.}\,\frac{1}{2}b + \text{cos.}\,\left(a + \frac{n+1}{2}b\right)\text{sin.}\,\frac{1}{2}nb}{\text{sin.}\,\frac{1}{2}b} =$$

$$\frac{\text{cos.}\,(a + \frac{1}{2}nb)\,\text{sin.}\,\frac{1}{2}(n + 1)b}{\text{sin.}\,\frac{1}{2}b}. \quad \text{(II.)}$$

For sin. $\frac{1}{4} b \times$ cos. $a = \frac{1}{4}$ sin. $(a + \frac{1}{4} b) - \frac{1}{4}$ sin. $(a - \frac{1}{4} b)$

And sin. $\frac{1}{4} n b \times$ cos. $(a + \frac{1}{4} b + \frac{1}{4} n b) = \frac{1}{4}$ sin. $(a + \frac{1}{4} b + n b) - \frac{1}{4}$ sin. $(a + \frac{1}{4} b)$

Therefore cos. $a \times$ sin. $\frac{1}{4} b +$ cos. $\left(a + \frac{n+1}{2} b\right)$ sin. $\frac{1}{4} n b$

$= \frac{1}{4}$ sin. $(a + \frac{1}{4} b + n b) - \frac{1}{4}$ sin. $(a - \frac{1}{4} b)$

But sin. $(\frac{1}{4} b + \frac{1}{4} n b)$ cos. $(a + \frac{1}{4} n b) = \frac{1}{4}$ sin. $(a + \frac{1}{4} b + n b) - \frac{1}{4}$ sin. $(a - \frac{1}{4} b)$.

Hence the equation marked (II.) is evident.

Corollary.—Cos. $a +$ cos. $(a + b) +$ cos. $(a + 2 b) +$ cos. $(a + 3 b)$ &c. ad infinitum $= - \dfrac{\text{sin. } (a - \frac{1}{4} b)}{2 \text{ sin. } \frac{1}{4} b}$.

For in this case $n b$ is infinitely great; therefore $a + \frac{1}{4} b + n b$ is infinitely great; but the sine of an infinitely great arc *may be* zero; therefore the sum of the infinite sines $= \dfrac{- \frac{1}{4} \text{ sin. } (a - \frac{1}{4} b)}{\text{sin. } \frac{1}{4} b}$

$= - \dfrac{\text{sin. } (a - \frac{1}{4} b)}{2 \text{ sin. } \frac{1}{4} b}$.

ARTICLE VII.

On the retrograde Variation of the Compass.
By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR,   *Bushey Heath, Stanmore, April 5, 1820.*

HAVING completed three years' observations on the diurnal variation of the magnetic needle, I have, for the purpose of readily comparing the results, collected in a table the mean monthly variation; referring such of your readers as wish for more specific information to those numbers of the *Annals of Philosophy* which contain the daily observations, accompanied by a meteorological table.

By examining this general table, it appears that during the first nine months of the second year's observation, the variation increased; fluctuated during the month of January, 1819; decreased in February, and again fluctuated in March of the same year, since which period the corresponding mean monthly variation has continually decreased, which circumstance induces me to conclude that the maximum of the western declination occurred about the month of March, 1819. By turning to the first volume of the *Annals of Philosophy*, it will be found, that in the year 1657, the compass had no variation, or pointed to the true North; consequently the lapse of time between the magnetic needle being at zero, and its arrival at the greatest western boundary, is 162 years. In the same number of the *Annals*, it is also recorded, that the declination of the needle in 1580 was

7

11° 15′ E.; but as it is not stated whether this was its greatest excess, the discovery of the maximum of eastern variation must be left to the assiduity of future observers.

As the observations at this place prove that the needle is now retrograde, it is to be regretted that others, with proper instruments, have not been made in different parts or the world, as they would clear up a point, at present questionable, whether the increase and decrease of the variation is simultaneous in every part of the globe.

The only observations which, I believe, have been published are those of the Royal Society, commenced by the late Mr. Gilpin, and continued by the present librarian; but notwithstanding the accuracy of the former, and the well-known scientific abilities of Mr. Lee, these observations being made in a room in which iron has been used to strengthen the ceiling (and not in the open air), it is doubtful whether the real variation can be truly ascertained. Magnetical observations have, I believe, lately commenced in two places; at the observatory of Glasgow, and at Chatillon, in Burgundy. Monsieur le Duc de Ragusa, under whose auspices the latter are conducted, instead of allowing the needle to turn on a point, suspends it by a thread (fil); but what advantages attend this alteration remain to be proved.

Two needles were always used for finding the variation; when one set of observations was made, the needle was unhung, replaced by a second, and a new series commenced; and when both were completed, the mean result was considered as the true variation; the first needle was then laid on a third, with the north end of the one in contact with the south end of the other, as by this means the magnetic power is preserved, if not increased.

The meteorological journal during the last months is not so full as it was my intention to have made it: the blanks in the column entitled " Velocity " were caused by the frequent breaking of the machines with which the wind was measured. The machine best adapted for this purpose, provided the rate does not exceed 30 miles per hour, is described in the *Annals* for October, 1818. The other was constructed like a windmill, and a table of the velocity of the sails is given in the *Annals* for Aug. 1816: this also, owing to the vast rapidity with which it moved in a strong wind, met with repeated accidents, which caused me to discontinue that part of the table.

The only merit I claim for these observations is industry, and a scrupulous attention to truth; and should they be found useful in throwing any light on this branch of natural knowledge, I shall have done no more than my duty towards that learned Society of which I have the honour of being a member.

I remain, my dear Sir, yours, very sincerely,

MARK BEAUFOY.

Table containing the Morning, Noon, and Evening Monthly Mean Variation of the Magnetic Needle for three Years.— Variation West.

Month	Time	1817			1818			1819		
April	Morn	24°	31'	52"	24°	34'	06"	24°	32'	36"
	Noon	24	44	43	24	44	50	24	43	09
	Even	24	35	58	24	36	36	24	34	59
May	Morn	24	32	20	24	36	18	24	32	42
	Noon	24	42	35	24	45	49	24	41	22
	Even	24	34	45	24	38	35	24	34	10
June	Morn	24	31	09	24	33	47	24	31	28
	Noon	24	42	14	24	45	11	24	41	41
	Even	24	34	45	24	37	40	24	35	09
July	Morn	24	31	14	24	34	24	24	32	31
	Noon	24	42	06	24	44	59	24	42	12
	Even	24	35	43	24	38	14	24	35	37
Aug.	Morn	24	31	16	24	34	40	24	32	33
	Noon	24	42	51	24	45	58	24	42	49
	Even	24	33	45	24	37	50	24	34	24
Sept.	Morn	24	33	02	24	34	29	24	32	29
	Noon	24	41	36	24	45	22	24	41	35
	Even	24	34	38	24	37	28	24	33	27
Oct.	Morn	24	31	06	24	35	36	24	33	27
	Noon	24	40	46	24	43	28	24	40	08
	Even	—	—	—	—	—	—	—	—	—
Nov.	Morn	24	31	49	24	33	24	24	32	42
	Noon	24	37	55	24	41	41	24	38	43
	Even	—	—	—	—	—	—	—	—	—
Dec.	Morn	24	34	03	24	37	04	24	33	29
	Noon	24	38	02	24	41	20	24	37	20
	Even	—	—	—	—	—	—	—	—	—
		1818.			**1819.**			**1820.**		
Jan.	Morn	24	34	02	24	35	42	24	34	06
	Noon	24	39	57	24	39	54	24	37	54
	Even	—	—	—	—	—	—	—	—	—
Feb.	Morn	24	34	22	24	34	17	24	32	19
	Noon	24	40	51	24	39	55	24	38	07
	Even	—	—	—	—	—	—	—	—	—
March	Morn	24	33	18	24	33	18	24	30	47
	Noon	24	41	37	24	41	42	24	39	33
	Even	24	33	47	24	35	17	24	33	45

Table containing the Mean Variation of each twelve Months' Observations.

	Time				Time				Time		
1st year	Morn	24°	32'	28"	2d year	24°	34'	45"	3d year	24°	32'	36"
	Noon	24	41	14		24	43	21		24	40	23
	Even	24	34	46		24	37	20		24	34	30

Article VIII.

Analysis of Petalite, and Examination of the Chemical Properties of Lithia.* By Dr. C. G. Gmelin, of Tubingen.

In vol. lix. p. 241, of the Annalen der Physik, there is a description of the external characters of the petalite, and an account of some of its properties by Dr. Clarke.† I shall make the following additions to this description. I found the specific gravity of petalite weighed in water of the temperature of $56\frac{3}{4}°$ $= 2·4268$.

When very thin scales of it are exposed to the action of the blow-pipe, they melt easily, and we then perceive with the naked eye that the surface of the melted matter is full of small vesicles.

The phosphates dissolve it with difficulty. The melted glass bead is transparent as long as it is hot, but on cooling, it becomes opaque. It is colourless, and even the addition of saltpetre (provided the petalite was pure) does not betray the presence of oxide of manganese in it; which it undoubtedly would do, if petalite contained, according to the statement of Clarke and Holme, 2·5 per cent. of oxide of manganese.

Borax dissolves it with facility. The bead is transparent and colourless. Carbonate of soda likewise dissolves it with facility.

I wished, before undertaking the analysis of petalite, to analyze some of the salts of lithia which are obtained during the analysis of petalite. My first business, therefore, was to obtain sulphate of lithia.

Some difficulty occurs when we analyze petalite by means of barytes. If we employ carbonate of barytes, it is necessary to mix a very great proportion of it with the petalite; and even then the decomposition is apt to be incomplete, unless the petalite has been reduced to an exceedingly fine powder. If we employ nitrate of barytes, a silver crucible is scarcely able to withstand the degree of heat necessary to produce a complete decomposition. After having examined both methods, it seemed to me most adviseable to expose the mixture of petalite and nitrate of barytes to a red heat in a platinum crucible, though I was aware that platinum is attacked by caustic barytes.

A portion of petalite reduced to a very fine powder by washing was triturated with five times its weight of crystallized nitrate of barytes, and exposed to a white heat for two hours in a platinum crucible. The mass, when treated with muriatic acid, appeared to have been completely acted upon; though, as is commonly

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* Translated from Gilbert's Annalen der Physik, lxii. 599.
† Translated from the *Annals of Philosophy*, xi. 194 and 363.

the case when minerals are decomposed by means of barytes, the silica was not entirely dissolved, a considerable portion of it appearing under the form of light transparent white flocks. The solution was evaporated to dryness in a platinum capsule, and the silica separated by the filter. After this, a greater portion of sulphuric acid was added to the liquid than was necessary to precipitate the whole of the barytes. The sulphate of barytes was separated by the filter, and the liquid was evaporated in a platinum capsule till it was reduced to a small quantity in order to get rid of the greatest part of the excess of muriatic acid which was present. It was then supersaturated with carbonate of ammonia; and after the precipitate, consisting probably of alumina and a little oxide of iron, had been separated by the filter, it was evaporated to dryness in a large platinum crucible, and heated to redness, in order to drive off the muriate and sulphate of ammonia which it contained. The residual mass was redissolved in water, and the solution mixed with hydrosulphuret of ammonia as long as any black precipitate consisting of sulphuret of manganese continued to fall. The filtered solution was again evaporated to dryness, and exposed to a red heat. The sulphate thus obtained was dissolved in water, and decomposed by means of acetate of barytes. The sulphate of barytes was separated by the filter. The liquid was evaporated to dryness, and the residual mass heated to redness in a platinum crucible. The matter thus obtained exhibited strongly the properties of an alkali even after it had been well washed with boiling water. The solution of carbonate of lithia in water was evaporated till the greatest part of the salt was deposited in the state of a crystalline powder, over which a very small portion of liquid swam. This crystalline powder was thrown into a funnel to allow the liquid to drop from it, and it was likewise washed with cold water, in order to remove any trace of soda with which it might possibly have been contaminated. I must, however, remark, that in my experiments on petalite, I have never met with any traces of soda.

A portion of this pure carbonate of lithia was converted into a sulphate by means of sulphuric acid, and rendered neutral by exposure to a strong red heat. It was then dissolved in water and crystallized.

My analysis of this salt gives its composition as follows :

Sulphuric acid	58·34	68·15
Lithia	27·25	31·85
Water	14·41	
	100·00	100·00

Its comes very nearly to the numbers given by Vauquelin in his analysis (Ann. de Chim. et de Phys. vii. 287). The composition of the salt, as given by that chemist, is as follows :

Sulphuric acid. .... 69·2
Lithia .... 31·8

———
100·0

But it is obvious that there is a mistake in these numbers; the number for lithia, instead of 31·8, should have been 30·8. From my analysis, it follows that lithia is composed of

Lithium . ... 58·05
Oxygen . ... 41·95

———
100·00

Chemical Analysis of Petalite.

(A.)

a. One hundred parts of petalite, when exposed to a red heat, were reduced to 97·83 parts. Hence the loss of weight amounted to 2·17.

b. 97·83 (2 grammes) parts of petalite, which had been exposed to a red heat, were mixed with four times their weight of carbonate of potash, and exposed for two hours to a strong red heat in a platinum crucible. The fused mass was dissolved in muriatic acid, and the solution evaporated to dryness in a platinum cup. The residual powder was boiled in water acidulated with muriatic acid, and the silica separated by the filter. After being washed, and dried, and heated to redness, it weighed 73·37.

c. After the separation of the silica, the solution was decomposed by caustic ammonia; this threw down a white precipitate, which, when subjected to the requisite trials, exhibited the properties of alumina. After being washed, dried, and heated to redness, it weighed 17·41.

d. To the solution was now added a portion of oxalate of ammonia. A precipitate fell consisting of oxalate of lime, which, in order to convert it again into carbonate of lime, was exposed to a red heat, and then heated in contact with carbonate of ammonia. The quantity of carbonate of lime thus obtained was equivalent to 0·32 of pure lime.

e. The residual solution was now mixed with carbonate of potash, and boiled. A precipitate fell, which might have been considered as magnesia; but it was insoluble in sulphuric acid, and exhibited the properties of silica. Its weight was 0·8.

Thus the constituents of 100 parts of petalite by the preceding analysis are as follows:

Silica, *b* ...... 73·37
        e ...... 0·80
                    ———
                    74·17 .... 74·17
Alumina, *c*. .... ·17·41
Lime, *d*. .... 0·32
Moisture, *a* .... 2·17
                                     ———
                                     94·07

(B.)

To determine the portion of lithia contained in petalite, 100 parts (4 grammes) of the mineral in fine powder were mixed with 800 parts of carbonate of barytes, and exposed for two hours to a white heat in a platinum crucible. The mass did not dissolve completely in muriatic acid, but it was obvious from the appearance of the residual matter, that it had been completely acted on by the barytes. The solution was evaporated to dryness, and the silica separated. It weighed 77·5 parts.

Thus in the present experiment, the quantity of silica somewhat exceeds that found in the preceding one..

After the separation of the silica, the liquid was mixed with a greater proportion of sulphuric acid than was requisite to throw down the barytes : after which the alumina and lime were thrown down by digestion with carbonate of ammonia.

The filtered solution was, in the first place, reduced to a small quantity by evaporation in a platinum cup. It was then evaporated to dryness, and heated to redness in a large platinum crucible to get rid of the muriate and sulphate of ammonia. There remained 16·2 parts of fused sulphate of lithia ; equivalent to 5·16 parts of lithia.

Hence the constituents of petalite are as follows ;

Silica. ...., 74·17
Alumina. .... 17·41
Lithia. .... 5·16
Lime.. ...., 0·32
Moisture. .... 2·17
                           ———
                           99·23
Loss ...., 0·77
                           ———
                           100·00

This analysis differs considerably from that made by Messrs. Clarke and Holme. Besides their not having found any indication of the presence of an alkali, the quantity of oxide of manganese which they detected is by far too great. In perfectly pure specimens of petalite, which I selected for my analysis, I could detect no manganese whatever ; though its presence was obvious

in less pure specimens of a pale rose red colour, which I generally laid aside as not sufficiently pure for my purpose.

Chemical Properties of Lithia.

The greater number of these properties are already known from the experiments of Arfvedson and Vauquelin. I have extended my researches somewhat further; and I have sometimes obtained results a little different from these chemists.

Caustic Lithia.—Into a solution of sulphate of lithia in a glass tube furnished with a glass stopper barytes water was dropped as long as a precipitate continued to fall. Much patience was necessary to obtain a liquid neither precipitated by barytes water, nor by sulphuric acid. The solution of caustic lithia was filtered as rapidly as possible into a tubulated retort, to which a receiver had been previously adjusted. The solution was then distilled by placing the retort on a sandbath. When the liquid in the retort became very concentrated, a white powder separated, and likewise some small granular crystals whose form could not be determined. These fell while the liquid was still hot.

The retort was now taken from the fire and placed in a cellar without exposing the liquid which it contained to the action of the air. Neither the powder nor the small crystals appeared to increase during the cooling. Hence it would appear that caustic lithia is not much more soluble in hot than in cold water. The very concentrated solution was evaporated to dryness in a platinum crucible by means of a spirit lamp. By this means dry caustic lithia was obtained in which acids could detect no trace of carbonic acid.

A portion of this matter exposed to heat in a platinum crucible melted before it became sensibly red-hot. The fused mass was transparent. When allowed to cool in the open air so as to absorb carbonic acid, it became opaque.

Caustic lithia has a very sharp burning taste. It destroys the cuticle of the tongue, like potash, and seems to me in point of causticity very nearly to equal that alkali. It does not dissolve with great facility in water. But the small quantity with which I was provided did not allow me to determine the exact solubility of it in that liquid. It appears, as has been already observed, not to be much more soluble in hot than in cold water. In this respect it has an analogy with lime. Heat is evolved during its solution in water.

When exposed to the air, it does not attract moisture, but absorbs carbonic acid, and becomes opaque. When exposed for an hour to a white heat in a covered platinum crucible, its bulk does not appear to be diminished; but it has absorbed a quantity of carbonic acid.

It dissolves only in small quantity in alcohol of the specific gravity 0·85. When weak alcohol is added to an aqueous solu-

tion of lithia in a well-stopped phial, no change takes place at first ; but after some hours, the lithia precipitates in the state of a white powder.

Lithia unites with sulphur, according to Vauquelin. Sulphuret of lithia has a yellow colour, dissolves readily in water, and is decomposed by acids in the same way as the other alkaline sulphurets.

Phosphorus decomposes water with the help of caustic lithia. If we heat in a retort phosphorus with a solution of caustic lithia in water, phosphuretted hydrogen gas is disengaged, which catches fire when it comes into the air. I have not attempted to form a solid combination of phosphorus and lithia.

Neutral Sulphate of Lithia forms small prismatic crystals, having a good deal of lustre, sometimes constituting pretty long, but narrow tables. When exposed to the air, they only undergo an insignificant efflorescence. This salt has a saline and scarcely bitter taste. It dissolves pretty readily in water, and melts when exposed to a temperature, scarcely reaching a red heat.

Bisulphate of Lithia dissolves in water with greater facility than the neutral salt. It forms six-sided tables, in which two of the faces, which are parallel to each other, far exceed the remaining ones in length. When exposed to a very high temperature, it gives out sulphurous acid and oxygen gas, and is converted into the neutral sulphate.

According to Arfvedson, this bisalt dissolves with more difficulty in water than the neutral salt.

Phosphate of Lithia.—Phosphoric acid, when dropped into the solution of sulphate of lithia, occasions no precipitate. But when the uncombined acid is saturated by ammonia, the phosphate of lithia is precipitated in the state of white flocks, which are insoluble in water.

When a drop of phosphoric acid is let fall into a very dilute solution of carbonate of lithia, no precipitate falls ; but when the liquid is heated, the carbonic acid gas is disengaged, and phosphate of lithia falls down. From this it would seem that the solubility of phosphate of lithia in water is owing to the presence of the carbonic acid.

There exists likewise a *biphosphate of lithia.* It is obtained by dissolving the neutral salt in phosphoric acid. By a very slow evaporation of this solution, we obtain transparent granular crystals.

Nitrate of lithia forms four-sided prisms with rhomboidal bases. It has a very pungent taste, and seems to me to surpass almost all other salts in deliquescency. In a very hot day, it crystallized in the sun ; but deliquesced again in the shade. It dissolves in the strongest alcohol.

Carbonate of lithia constitutes a white powder. It dissolves with great difficulty in cold water. According to Vauquelin, 100 parts of water scarcely dissolve one part of this salt. It is more

soluble in hot water. A solution of carbonate of lithia containing only $\frac{1}{1000}$th of its weight of the salt, acts strongly as an alkali.

0·535 gramme of fused carbonate of lithia were, by means of sulphuric acid and exposure to a strong heat, converted into 0·765 of neutral sulphate of lithia. Now this quantity of sulphate contains 0·2436 of lithia.

Hence 0·535 of carbonate of lithia are composed of

$$
\begin{array}{lr}
\text{Lithia} \dots\dots\dots\dots\dots\dots & 0\text{·}2436 \\
\text{Carbonic acid} \dots\dots\dots\dots & 0\text{·}2914 \\
\hline
& 0\text{·}5350
\end{array}
$$

Or in the 100 parts,

$$
\begin{array}{lr}
\text{Lithia} \dots\dots\dots\dots\dots\dots & 45\text{·}54 \\
\text{Carbonic acid} \dots\dots\dots\dots & 54\text{·}46 \\
\hline
& 100\text{·}00
\end{array}
$$

But the oxygen in

$$
\begin{array}{lr}
45\text{·}54 \text{ lithia is} \dots\dots\dots & = 19\text{·}09 \\
54\text{·}46 \text{ carbonic acid.} \dots & = 39\text{·}59
\end{array}
$$

and $2 \times 19\text{·}09 = 38\text{·}18$, a number differing but little from $39\text{·}59$. This analysis, therefore, corresponds very nearly with the law discovered by Berzelius, according to which, in all neutral carbonates, the oxygen in the acid is double the quantity of that in the base.

To ascertain whether a bicarbonate of lithia can be formed, I caused a stream of carbonic acid gas to pass in a cool place through a solution of carbonate of lithia in which there was a quantity of undissolved carbonate. This last portion was speedily dissolved by the carbonic acid; but I could not obtain a single crystal of bicarbonate. On exposing the liquid to a very moderate heat, a quantity of carbonic acid gas was disengaged, and, when the liquid cooled, the neutral carbonate of lithia was again precipitated.

The solution of carbonate of lithia is decomposed by lime and barytes water. It is insoluble in alcohol.

There dissolves an exceedingly small portion of carbonate of glucina, even when the carbonate of glucina is dissolved in muriatic acid, and the solution boiled with an excess of carbonate of lithia.

I made the observation that the platinum crucible in which carbonate of lithia has been exposed to a red heat gives obvious indications of having been attached, its surface assuming a dark olive-green colour; but the metallic lustre is again restored by rubbing the crucible with coarse sand and water.

Muriate of lithia forms small regular cubes very similar to common salt in their taste. The easiest method of obtaining the

crystals is to expose the solution to the sun in a hot day.* The crystals deliquesce very speedily, when exposed to the air, but not with so much rapidity as nitrate of lithia. This salt does not melt when exposed to the red heat produced by the action of a spirit lamp; but when exposed in a platinum crucible, not completely covered, to an incipient white heat, it is fused.

Borate of Lithia.—The aqueous solution of boracic acid does not easily decompose the solution of carbonate of lithia at a boiling heat. After the evaporation of the liquid, the borate of lithia is obtained in the state of a gummy, transparent matter, which absorbs moisture in a damp place, and dissolves with facility in water.

The *biborate of lithia* forms a crystalline mass. I have not been able to determine the shape of the crystals; though I observed some three-sided pyramids, and some granular crystals. These crystals do not dissolve readily in water; yet they seem to be more soluble than boracic acid. When acetic acid is poured into the solution, boracic acid is immediately precipitated.

During these experiments I made an observation, which I shall mention here, as I do not know whether it be generally known. The solution of boracic acid in water gives a reddish-brown colour to turmeric paper, prepared from a spirituous tincture of turmeric root, pretty much as a weak alkaline solution would do; while it acts in the usual manner upon litmus paper. To render the brownish-red colour of the turmeric paper apparent, it must be allowed to dry.

Chromate of Lithia forms orange-yellow crystals, which appear to contain an excess of acid. They are oblique parallelopipeds with rhomboidal bases. Sometimes they exhibit dendritical vegetation. This salt is soluble in water.

Tungstate of Lithia.—When carbonate of lithia is boiled with tungstic acid, previously exposed to a red heat (prepared from wolfram by means of muriatic acid and ammonia), carbonic acid is disengaged, and we obtain a compound of tungstic acid and lithia; but the decomposition of the carbonate of lithia takes place very slowly. Tungstate of lithia forms very large crystals consisting of oblique low prisms with very oblong rhomboidal bases. The taste of this salt is first sharp, then sweet, and finally astringent. It appears to be pretty soluble in water.

Oxalate of Lithia.—A portion of carbonate of lithia was saturated with oxalic acid. The neutral salt crystallizes with difficulty. The crystals have the appearance of small opaque protuberances, and dissolve with facility in water. To the neutral solution of oxalate of lithia was added a quantity of oxalic acid exactly equal to that already combined with the

* Arfvedson did not bring the salt to crystallization,

lithia. By evaporation, small transparent granular crystals of *binoxalate of lithia* were obtained. They appeared to dissolve with facility in water, though not to be so soluble as the neutral salt.

Neutral *tartrate of lithia* dissolves with facility in water, but does not crystallize, forming a white opaque mass, which does not deliquesce when exposed to the air. When tartaric acid is added to the solution of the neutral tartrate, no crystallizable bitartrate is formed; but perhaps we may deduce the existence of such a salt from the fact that when the solution is evaporated no crystals of tartaric acid make their appearance.

When the solution is evaporated to dryness, we obtain a white opaque mass, which exhibits no appearance of crystallization, and which dissolves with facility in water.

Acetate of Lithia, when evaporated, forms a syrupy mass, which cracks on cooling; so that the glass looks as if it had burst. This matter deliquesces in the air, and sometimes, while attracting moisture, crystalline plates appear in it.

Gallate of Lithia.—Gallic acid * decomposes the carbonate of lithia with great facility. It forms a dark-green solution, which possesses a strong colouring power. The concentrated solution, even when air is excluded, assumes a brownish, and at last almost a blood-red colour. When evaporated in the air, gallate of lithia remains in the state of a black matter which does not crystallize. A very dilute solution of gallate of lithia, when left exposed to the air, becomes gradually darker and darker, and a brownish powder separates from it.

Benzoate of Lithia.—Benzoic acid readily decomposes carbonate of lithia, and forms a salt, very soluble in water, which does not crystallize, but constitutes a white opaque mass, which does not deliquesce. When heated to redness, it is converted into carbonate of lithia, and it leaves behind it a very bulky charcoal.

Saclactate of Lithia.—Saclactic acid † readily decomposes carbonate of lithia when assisted by heat. When the solution is slowly evaporated, it deposits white, brilliant needles, which are rather deliquescent, and dissolve readily in water.

Malate of Lithia.—Malic acid ‡ decomposes the carbonate of lithia with great facility; but I was neither able to obtain a malate nor bimalate of lithia. Both form a syrupy mass, which did not become dry when heated. Indeed it was not in my power to obtain malic acid in the state of crystals.

But when carbonate of potash was added to the same acid, so that there was still an excess of acid in the solution, it was easy to obtain crystals of bimalate of potash. They consisted of

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* It was prepared by the new process of Braconnot.—(See Ann. de Chim. et de Phys. ix. 181.)

† It was prepared from sugar of milk by means of nitric acid.

‡ It was prepared by decomposing pure sorbate of lead by means of sulphuretted hydrogen gas.

collections of small needles, usually radiating from a common centre.

Double Salts.

Tartrate of Lithia-and-Potash.—If the excess of acid of bitartrate of potash be saturated by means of carbonate of lithia, we obtain, by spontaneous evaporation, a salt which forms large crystals, having the shape of four-sided prisms terminated by parallelograms, with angles very nearly right. The diagonals of these terminal faces are distinctly marked, and the four triangles formed by means of them are streaked parallel to the edges of these faces. I dissolved this salt repeatedly in water, and crystallized it again, and always obtained the very same kind of crystals; so that, by this property, lithia seems to be very well characterized. This salt dissolves readily in water, and has a saline and scarcely bitter taste. When exposed to the air, it effloresces slightly, and only on the surface.

Tartrate of Lithia-and-Soda.—Bitartrate of soda was neutralized by means of carbonate of lithia.' By spontaneous evaporation, the liquid deposited long rectangular four-sided prisms, frequently terminated by an oblique face. This salt dissolves with facility in water, and effloresces only slightly and on the surface. Its taste is purely saline, and not strong.

Sulphate of Alumina-and-Lithia.—No crystallized salt analogous to alum can be formed by combining these substances together. I mixed three different times together solutions of pure sulphate of alumina and sulphate of lithia, and subjected the mixture to spontaneous evaporation. I always obtained a white, opaque mass, not crystallized. This does not agree with the experiments of Arfvedson, who obtained crystals by the spontaneous evaporation of such a mixture, which had very much the appearance and taste of alum.

Muriate of Platinum does not form a double salt with muriate of lithia. Potash and lithia, therefore, may be very well distinguished from each other by means of muriate of platinum.

Attempt to procure Lithium the Basis of Lithia.

A small portion of caustic lithia still remaining in my possession, I endeavoured to procure from it the combustible substance which constitutes its basis; but as I was not in possession of a very powerful galvanic battery, I tried whether I could not form an amalgam of it with mercury. I employed for the experiment 182 pairs of copper and zinc plates, the surface of each of which amounted to $3\frac{1}{4}$ French inches square. The caustic lithia was put into a small platinum cup, and brought by means of water to the consistence of pap. Into this matter a globule of mercury was put, which was brought into contact with the negative pole of the battery; while the platinum cup was in contact with the *positive* pole. The contact was effected by means of platinum *wires.*

In a short time a change was perceived in the globule of mercury. It had become larger, and had assumed a red colour; and on the under side of the globule, which was in contact with the lithia, a black crust soon appeared. I put both the globule and the black crust into rectified naphtha.

I distilled off the mercury in an apparatus similar to that described by Davy, in his paper on the bases of the alkaline earths. The only peculiarity in my apparatus consisted in this, that from the hollow glass ball which served the purpose of a receiver, there passed a tube of safety which plunged into mercury. The whole distilled over without leaving any residue, and the water in contact with which the mercury had been put did not possess alkaline properties. It would appear from this that the metallic basis, as fast as it was formed, had been again converted into an oxide. The black crust, which existed only in minute quantity, was probably a mixture of suboxide of lithium and oxide of mercury; for, when heated with water, its quantity was diminished, the water exhibited alkaline properties, and an oxide of mercury remained behind.

Sir Humphry Davy has obtained lithium, and found it to resemble sodium in its properties.

———

From the preceding account of the salts of lithia, we see that they have many properties in common with the salts of soda. Like them, they are neither precipitated by muriate of platinum, nor by tartaric acid. They may, however, be distinguished from the salts of soda by the following properties: When their concentrated solutions are mixed with a concentrated solution of carbonate of soda, a precipitate falls. They are likewise precipitated by phosphate of soda and phosphate of ammonia, when no uncombined acid is present.

In reference to analytical chemistry, I may remark that lithia, potash, and soda, if they should exist in the same compound, may be separated in the following way:

Lithia may be precipitated by means of phosphoric acid and an excess of caustic ammonia. The phosphate of lithia may be dissolved in acetic acid, and the phosphoric acid precipitated by means of acetate of lead, &c.

When lithia exists in a compound along with potash, this last alkali may be precipitated by means of muriate of platinum.

From the results of the preceding experiments, we see that if 10 be the equivalent number for oxygen, the equivalent number for lithium is 13·83, and for lithia 23·83; that for carbonate of lithia by calculation 51·32; but, according to the preceding experiments, 52·32; &c.

ARTICLE IX.

Experiments to determine the Composition of different inorganic Bodies which serve as a Basis to the Calculations relative to the Theory of Chemical Proportions. By J. Berzelius.

(*Continued from* p. 283.)

Composition of the Acids of Arsenic; their Capacity of Saturation; Oxide and Sulphurets of Arsenic.

THE experiments of the greater number of chemists agree in general that arsenic absorbs about a third of its weight of oxygen to become arsenious acid, and half its weight to become arsenic acid. Such also was the result of some experiments which I made òn the subject long ago. I conceived that I ascertained at the same time that the capacity of saturation of arsenic acid was 16·66, and that consequently this acid contained twice as much oxygen as the base with which it was combined. But the same observations which induced me to make a new examination of the acids of phosphorus led me to undertake experiments on the acids of arsenic. These first experiments gave me inaccurate results. I found that the capacity of saturation of arsenic acid, in its neutral combinations, was 13·8, a number which was not a submultiple of the oxygen of the acid as I had found in the other acids. Hence I concluded that the analyses had given inaccurate results; and the more so as M. Laugier had found that 100 arsenic combine with 71·8 sulphur; a quantity which does not agree in any manner with the ordinary ratio between sulphur and oxygen in the other metals. I, therefore, had recourse to the analytical method, as synthesis had hitherto been employed to determine the composition of these acids. I mixed arsenious acid with sulphur, and I heated the mixture in a retort till the acid was completely converted into sulphuret. One hundred parts of arsenious acid lost in sulphur and in oxygen 61 parts, of which 30¼ must have been oxygen. Making use of this analysis as the basis of my calculations, and admitting that arsenic forms arsenic acid with one and a half times as much oxygen, it follows that in the neutral arseniates the acid contains three times the oxygen of the base, and in the subarseniates twice that quantity. The analysis of sulphuret of arsenic by M. Laugier agreed perfectly with the composition of an oxide of arsenic, in which the radical is combined with half the oxygen contained in the acid, and it induced me to consider the existence of such an oxide probable. This almost perfect coincidence surprised me so agreeably that I thought it unnecessary to repeat the analytical experiment. The result of this experiment, however, was inaccurate, owing to a cause to which I did *not at the time* attend. The experiment was made in a retort, of

which the mixture did not fill more than a fifth part. The consequence was, that a portion of the sulphur was oxidized at the expense of the air of the retort, and thus considerably increased the loss of sulphur.

Having found that the acids of phosphorus follow different laws from all the other acids that I had occasion to examine, I thought it likely that the acids of arsenic might follow the same laws. This idea induced me to resume the examination of these acids.

One hundred parts of arsenic were acidified by means of nitro-muriatic acid, and the liquid was evaporated till the volatile acids were driven off. The arsenic acid remaining was dissolved by water acidulated with a little nitric acid, and 400 parts of oxide of lead, just exposed to a red heat, were added to it. This mixture was allowed to digest for some hours. It was then evaporated to dryness, and exposed to a red heat till no more nitrous vapours were exhaled. The oxide of lead had been added that the presence of water in arsenic acid might not alter the result. This experiment was several times repeated, and it gave results which varied between 150 and 152 of arsenic acid produced from 100 of metal. This experiment, though not very precise in itself, proves, however, very nearly the dose of oxygen absorbed.

2·2715 grammes of pure arsenious acid were melted with an equal weight of pure sulphur in a small apparatus made by a glass blower. The bulb of this apparatus was entirely filled by the pulverized mass. The sulphurous acid gas passed through a long glass tube curved into a spiral, and whose mouth was shut by a piece of filtering paper to retain the flowers of sulphur, a small quantity of which, without this precaution, might have made its escape with the gas. I began by heating the bottom of the bulb by a very small oil lamp; and by this means the disengagement of the sulphurous acid began at the bottom, before the surface had acquired the temperature necessary to kindle the sulphur. Thus the sulphurous acid gas assumed the place of the small quantity of air in the tubes before it was decomposed. I then made use of a spirit of wine lamp, and the experiment was continued as long as any gas continued to be disengaged, which was from five to six hours. When the apparatus was cold, and the sulphurous acid gas replaced by air, the whole was weighed. It had lost 1·084 gramme.

2·203 grammes of arsenious acid in a similar experiment gave 1·069 of sulphurous acid. From these two experiments, the last of which was made with peculiar care, arsenious acid contains 24·176 per cent. of oxygen.

A gramme of arsenious acid dissolved by dilute muriatic acid was precipitated by a current of sulphuretted hydrogen gas. Care was taken to boil the liquid a quarter of an hour before introducing the gas. The precipitate had a fine lemon-yellow colour. It was collected on a filter and well washed. Dried at

a temperature a little above 212°, it weighed 1·245. From the quantity of oxygen absorbed by the hydrogen of the sulphuretted hydrogen gas, arsenious acid should contain 24·218 per cent. of oxygen. According to these experiments, 100 arsenic combine with 31·884 oxygen, and not with 33⅓, as has been hitherto believed. And as experiment proves that 100 arsenic absorb more than 50 to become arsenic acid, it follows that the ratio of the oxygen in the two acids of arsenic cannot be that of 2 to 3. It remains, therefore, to try, whether the ratio between the oxygen of phosphorous and phosphoric acids; that is to say, 3 to 5, does not hold likewise in the corresponding acids of arsenic. In this case 3 : 5 :: 31·884 : 53·14. If this is the case, we have reason to expect that the acids of arsenic will deviate from the laws of the combinations of oxided bodies in the same way as the acids of phosphorus. Let us then examine the capacity of saturation of arsenic and arsenious acids.

Arseniate of Lead.—A solution of nitrate of lead was mixed with a solution of arseniate of soda, with the precaution of not precipitating all the arsenic acid. The precipitate was washed, dried, and heated, to redness: 10 grammes of arseniate of lead decomposed by sulphuric acid produced 8·953 grammes of sulphate of lead. According to this experiment, arseniate of lead is composed of

Arsenic acid........ 34·14 ...... 100·00
Protoxide of lead.... 65·86 ...... 192·91

Now 192·91 parts of oxide of lead contain 13·834 of oxygen.

One part of arseniate of lead which had not been dried was digested with caustic ammonia, and then washed and dried at a red heat: 100 parts of subarseniate of lead thus obtained, decomposed by sulphuric acid, produced 101·6 parts of sulphate of lead. Hence this subsalt is composed of

Arsenic acid........ 25·25 ...... 100·00
Oxide of lead. ...... 74·75 ...... 296·04

But 192·91 × 1¼ = 299·3.* Hence it is evident that abstracting the errors of the analytical experiments, arsenic acid combines in the subsalt with one and a half times as much base as in the neutral arseniate.

Arseniate of Barytes.—It was prepared by pouring nitrate of barytes into a solution of crystallized arseniate of soda, to which a little arsenic acid was added; so that the alkaline reaction was removed as exactly as possible. No precipitate appeared at first; but after an interval of a few moments, the arseniate of barytes precipitated in the form of small crystalline scales, which were washed and dried at a red heat. Ten grammes of this arseniate

* 192·91 × 1½ = 289·36.—T.

of barytes produced 8·693 grammes of sulphate of barytes. Hence the salt is composed of

> Arsenic acid .... 42·94 ...... 100·00
> Barytes .... 57·06 ...... 132·88

These 132·88 parts of barytes contain 13·8867 parts of oxygen; which approaches very nearly to the analysis of arseniate of lead.

A portion of arseniate of barytes, which had not been dried, was digested in caustic ammonia, washed, dried, and exposed to a red heat. One hundred parts of this subsalt decomposed by sulphuric acid furnished 102·4 of sulphate of barytes. Hence the salt is composed of

> Arsenic acid .... 33·44 ...... 100·00
> Barytes .... 66·56 ...... 199·04

But $132·88 \times 1\frac{1}{2} = 199·32$. Hence it follows, that the acid is combined in this salt with $1\frac{1}{2}$ times as much salt as in the neutral arseniate.

Arsenite of Lead.—Arsenious acid was dissolved in caustic ammonia, and to separate all the excess of ammonia, the saturated solution was boiled for some time. It allowed a great deal of ammonia to fly off, and a crystalline deposit of arsenious acid took place. The liquid was left for some days in a temperature under 32° to get rid of all the excess of acid, which crystallized in octahedrons containing neither ammonia nor water. Then 10 grammes of pulverized and dry nitrate of lead were dissolved in water, and the arsenite of lead was poured into the liquid as long as any precipitate fell. The precipitate, being well washed and dried, was introduced into a glass retort, and fused. It produced a yellowish glassy mass, weighing 12·29 grammes. A small quantity of arsenious acid sublimed during the fusion, as always happens when the arsenites are exposed to heat; but its weight is included in the 12·29 grammes. To determine whether the whole oxide of lead of the nitrate was contained in the fused mass, I examined the liquid from which the arsenite had been precipitated. It became muddy on the addition of sulphuric acid, as was the case likewise with the water employed to wash the precipitate. The quantity of sulphate of lead thus obtained weighed 0·356 gramme, equivalent to 0·261 of pure oxide of lead, which of course must be subtracted from the 6·731 grammes of oxide of lead contained in the 10 grammes of nitrate of lead. It follows that 6·47 grammes of oxide of lead had produced 12·29 grammes of arsenite of lead. Of course the salt is composed of

> Arsenious acid ...... 47·356 ...... 100·00
> Oxide of lead ...... 52·644 ...... 111·17

But these 111·17 of oxide of lead contain 7·972 of oxygen.

A solution of subacetate of lead was precipitated by arsenite of

ammonia, prepared as above described, and care was taken not to precipitate the whole of the oxide of lead. The precipitate, well washed and dried, was fused in a glass retort. The vitreous matter thus obtained was reduced to a fine powder.* Ten grammes of this powder were dissolved in nitric acid, and then precipitated by sulphate of ammonia. The liquid was then neutralized by caustic ammonia. This occasioned a small additional precipitate, which was added to the former. The sulphate of lead, washed and dried at a red heat, weighed 9·32 grammes, equivalent to 6·87 grammes of oxide of lead; so that in this salt, 100 of arsenious acid were combined with 219·5 of oxide of lead; but 111·17 × 2 = 222·34. Hence it follows, that neglecting the inaccuracies of observation, the acid is combined in it with twice as much base as in the neutral salt.

From these experiments, it appears to follow that the capacity of saturation of arsenic acid is 13·886, and that in the subarseniates, the acid is combined with 1¼ times as much base as in the neutral arseniates. The capacity of saturation of arsenious acid is 7·972; and in the subarsenites, the acid is combined with twice as much base as in the neutral arsenites.

If arsenic acid, as we have stated above, is composed of 100 metal + 53·14 oxygen, it must contain 34·7 per cent. of that substance ; but if we suppose the quantity of oxygen in the acid to its capacity of saturation to be in the same ratio as we found for phosphoric acid; that is to say, that the acid contains 2¼ times as much oxygen as the base which it neutralizes, it follows, that the quantity of oxygen in the arsenic acid is 13·8867 × 2¼ = 34·717. Now this agrees perfectly well with experiment.

In arsenious acid, we have found from 24·176 to 24·218 per cent. of oxygen. Its capacity for saturation is 7·972, which, multiplied by 3 = 23·916. The difference ought obviously to be ascribed to inaccuracy of observation. In the subarsenite of lead, the acid contains 1¼ times as much oxygen as the base. The same ratio holds in the neutral phosphites.

If we admit that the analysis of arseniate of barytes gave an exact result, and if we admit further that arsenic acid contains 2¼ times as much oxygen as the base by which it is neutralized, the result calculated for the composition of arsenious acid agrees perfectly with that found by the analytical experiments already described. It follows then, that the acids ought to be composed thus :

	Arsenic acid.		Arsenious acid.	
Arsenic.	65·283	100·000	75·81	100·000
Oxygen.	34·717	53·179	24·19	31·907

* *The two arsenites* of lead, both before and after fusion, are so idio-electric that it is difficult to pound them in a mortar, without a great part of the powder being driven out of the mortar by the electrical repulsion.

The analogous manner in which the acids of phosphorus and arsenic deviate from the general law of the combinations of oxidized bodies is a very remarkable circumstance ; and so much the more so, as these acids have a great deal of analogy in other respects. Phosphoric and arsenic acids form with the alkalies crystallizable salts, which, though proportional to the other neutral arseniates and phosphates, have an alkaline reaction ; and if we saturate their solutions exactly by adding acid, and then concentrate the liquid by evaporation, the salt which crystallizes reacts as an alkali ; while an acid and less crystallizable salt remains in solution. Both arsenic and phosphorus give with hydrogen combinations which have not acid characters, and which in their chemical properties differ completely from the combinations of hydrogen with sulphur, selenium, and tellurium. Both give degrees of acidification, between which the ratio of the oxygen is as 3 to 5, and in both, the greater number of the anomalies would disappear, if what we consider as the simple radical contained a portion of oxygen, amounting to one-fifth of what it absorbs in order to become an acid in *ic*. But I have already, in speaking of phosphorus, shown, that at present we have no probable reason for believing that it contains oxygen ; and the probability is still smaller for arsenic, which possesses most of the characters of metals.

In the great number of the combinations of oxidized bodies which I have examined, the law of the multiples of the oxygen of the two oxides combined is always followed, except in the compounds formed by the acids of arsenic, phosphorus, and azote, when we consider this last as a simple substance.

In the acids formed by these radicals, the ratio of the oxygen of the acid in *ous* is to that of the acid in *ic* as 3 to 5. In the neutral arseniates and phosphates, the acid contains $2\frac{1}{2}$ times as much oxygen as the base, and this ratio exists likewise in some subnitrates. In all the arseniates, phosphates, and nitrates, the oxygen of the base is a certain submultiple by 5, or, more rarely, by 10, of the oxygen of the acid (with the exception of some phosphates of lime). This may give rise to the following questions : Does the law of multiples of oxygen in the two oxides combined (that is to say, that the oxygen of the one is a multiple by a whole number of the oxygen of the other) hold in all cases, except those in which the radical has two degrees of acidification, the oxygen of which is in the ratio of 3 to 5 ? What is the reason that in the compounds formed by these acids, it happens so rarely that the oxygen of the acid is a multiple by a whole number of the oxygen of the base ? It is evident that a satisfactory answer to these questions would be of the greatest importance to the theory of chemistry. It appears clear likewise that if we prove hereafter that azote is an oxide, which is probable, and which will be determined when the phenomena of the reduction of ammonia by the electric pile have been better studied and

8

explained—it appears clear, I say, that we shall then have a key to explain similar anomalies, exhibited even by the acids of phosphorus and arsenic; though such an explanation may have little probability at present.

Experiments rendering the Existence of an Oxide of Arsenic probable.—Some authors pretend that metallic arsenic exposed to the influence of the air falls into a black non-metallic powder; and Bergman advises us to keep arsenic under water. I had an opportunity of verifying this property of arsenic in an experiment in which 100 arsenic in a vessel covered with paper, after an interval of some months, had acquired almost eight parts in weight, and afterwards ceased entirely to acquire more. The black powder thus produced was insoluble both in water and in acids; but when digested in muriatic acid, it assumed the metallic lustre, and the acid was found to contain arsenious acid in solution. When strongly heated, it gave out in the first place arsenious acid, and then left a residue of metallic arsenic. In short, this powder possesses the characters of the class of oxides, to which I have ventured to give the name of *suboxides;* and which, without being able to combine with other oxidized bodies, are decomposed by very slight forces, a portion of their radical being reduced, while another is carried to a higher and more stable degree of oxidation.

As the quantity of oxygen found in this suboxide is a fourth of that of arsenious acid; without, however, being likewise a simple submultiple of the oxygen in arsenic acid, I wished to examine its composition anew; but to my great surprise, I was unable to procure arsenic, which falls in powder in the air. I have small quantities of metallic arsenic exactly weighed, which have remained for three years in glass vessels covered with paper, and which have not acquired any additional weight. I do not know what constitutes the difference between the metallic arsenic, which falls to powder, and that which remains unaltered, nor have I any idea of the different processes to be followed in order to obtain the one or the other.

I endeavoured then to produce an oxide of arsenic in combination with an acid. I heated arsenic in a phial filled with muriatic acid gas. The arsenic underwent no change; but a small quantity of puce-brown matter sublimed. The muriatic acid gas was not absorbed, and no smell of arseniuretted hydrogen gas was perceived when the gas was passed into the air. The brown matter was not altered by water; but on adding a little caustic potash, it detached itself immediately from the glass, recovered its metallic lustre, and fell into light scales which swam in the liquid. This experiment seems to indicate an action between muriatic acid gas and arsenic, though too weak to draw any certain conclusions from it. I then mixed *three* parts of calomel with one part of metallic arsenic, and *distilled* them together. There were formed in the first place

some drops of a liquid which distilled over, and which was the anhydrous combination of muriatic and arsenious acids. Then a sublimate rose of a deep red colour, which lined the inside of the tube; and lastly, an amalgam of arsenic covered the inside of the red sublimate. I separated them, and mixed the non-metallic mass with a new dose of metallic arsenic, and sublimed it anew by a very gentle heat. The sublimate was at first transparent, and of a fine red, inclining a little to yellow; but as it became thicker, it acquired a darker shade, and lost its transparency. The metallic arsenic remained in the phial. The sublimate was easily detached from the glass. Its colour was brown, its fracture earthy, without any marks of crystallization, and its powder yellow. It was insoluble both in water and muriatic acid. Copper rubbed with the powder moistened with muriatic acid was not attached. Mixed with iron filings, and exposed to heat, it gave out arsenic, which sublimed in crystals. The fixed caustic alkalies decomposed this mass almost instantly; and so did ammonia after a short interval. Muriate and arsenite of the alkali were found in the liquid, and there remained an amalgam of arsenic. Hence the sublimate was a double salt, having for its bases protoxide of mercury and oxide of arsenic, which, as happens with the oxides of sulphur and of phosphorus, is decomposed the instant it is disengaged, producing arsenious acid, and allowing a part of its radical to be reduced to the metallic state. In this case, the metallic arsenic had likewise reduced the mercury, so that its quantity was so much the more reduced.

I have no doubt that it is possible to obtain the combination of muriatic acid with oxide of arsenic without the presence of proto-muriate of mercury; but I have not made the necessary experiments either to determine this point, or to find the quantity of oxygen with which the arsenic is combined to constitute the oxide; so that I cannot say whether the oxide of the double salt of which I have just given the description, is the same as the black oxide formed by exposing metallic arsenic to the air.

Sulphurets of Arsenic.—Klaproth and Laugier made experiments on the native sulphurets of this metal, and each of them obtained the same results. M. Haüy having had reason to suspect that these two sulphurets have the same primitive crystalline form, he concluded that their chemical composition must also be the same, though the external characters were altered by some accidental mixture. M. Laugier, while occupied with these researches, found that the two native sulphurets, when heated in a phial, gave a sublimate of arsenious acid, which was more abundant from the red sulphuret than the yellow. The fused mass thus deprived of arsenious acid had always the same composition. He found in the red sulphuret 43·67 sulphur in 143·67 of sulphuret, in the yellow sulphuret 61·66 sulphur in 161·66 of sulphuret, and in the fused sulphuret, from which the arsenious acid had been driven, from 71·3 to 71·89 sulphur in

171·3 of sulphuret; that is to say, in 100 parts of metallic arse-
nic. Hence he concludes, agreeably to the conjecture of Haüy,
that these native sulphurets were probably combinations of the
sulphuret obtained by fusion with different quantities of arsenious
acid. The want of correspondence between the composition of
the sulphuret of arsenic obtained by fusion in the experiments of
Laugier, and that of the acids of this metal, induced me to exa-
mine these native sulphurets with this object in view, and to
avoid all error from combustion, I exposed them to heat in vessels
previously freed from air. The sulphuret melted and formed a
brown transparent liquid, which required a very strong heat to
be distilled over. I left it a long time at a temperature little
lower than that at which it boils, without any trace of arsenious
acid subliming. Afterwards, on increasing the heat, the sulphu-
ret boiled, and distilled over in yellow drops. In these experi-
ments, no trace of arsenious acid could be perceived; but on
making the same experiment in an open phial, the sulphuret
underwent a kind of roasting, in consequence of which sulphur-
ous acid gas and arsenious acid were disengaged, the former of
which made its escape, while the latter crystallized in the upper
part of the phial.

Let us now examine the composition of the native sulphurets
of arsenic. In the first place, they contain only sulphur and
arsenic. Laugier found that the yellow sulphuret contains 38·14
per cent. of sulphur. In the yellow brilliant substance obtained
by precipitating a solution of arsenious acid by sulphuretted
hydrogen gas, there are, according to experiments already
stated, 39 per cent. of sulphur; that is to say, that 100 parts of
arsenic are combined in it with 64·33 * of sulphur. We know
that orpiment is more or less sensibly mixed with realgar, which
probably is never wanting. This circumstance ought to diminish
the quantity of sulphur found in it by analysis. Hence the
reason why analysis gave 38·14 instead of 39 per cent. of this
last substance. Laugier and Klaproth found in realgar 100
arsenic united to 43·67 sulphur; but this quantity is very nearly
two-thirds of 64·33. The precise quantity should have been
42·9 ; but it is to be presumed that realgar contains a mixture of
orpiment, thereby containing a slight excess of sulphur; just as
we have seen the reverse to be the case with orpiment. I think
then that we may admit it as established that orpiment and
realgar are two different sulphurets of arsenic in which the quan-
tity of sulphur is as 1 : 1$\frac{1}{2}$, or as 2 : 3. The yellow sulphuret is
proportional to the arsenious acid, and the red sulphuret to a
degree of oxidation, which contains two-thirds as much oxygen
as arsenious acid, and which may be the oxide which we have
seen combined with arsenious acid.

The artificial sulphuret produced in the experiments of Laugier

* 63·93.—T.
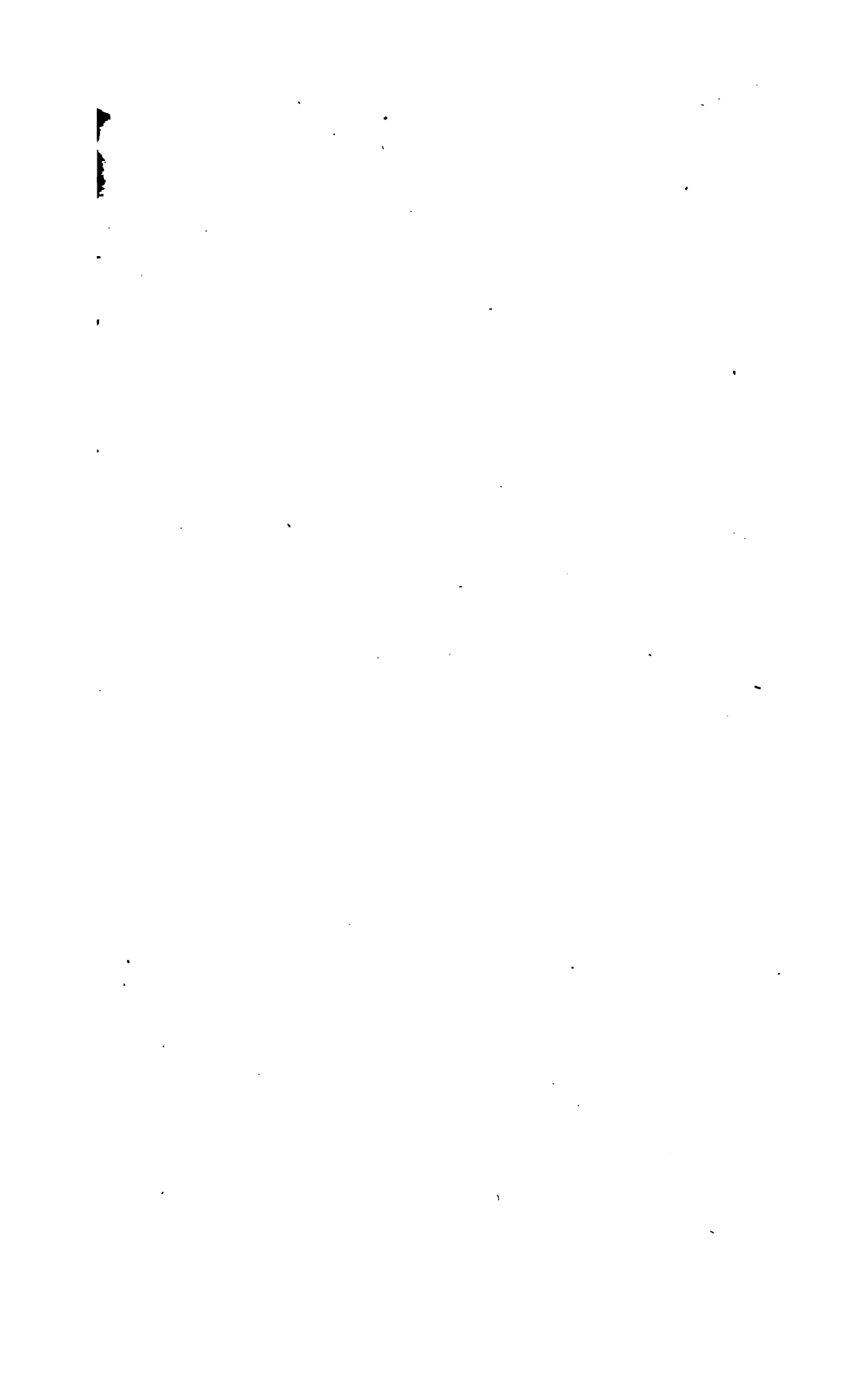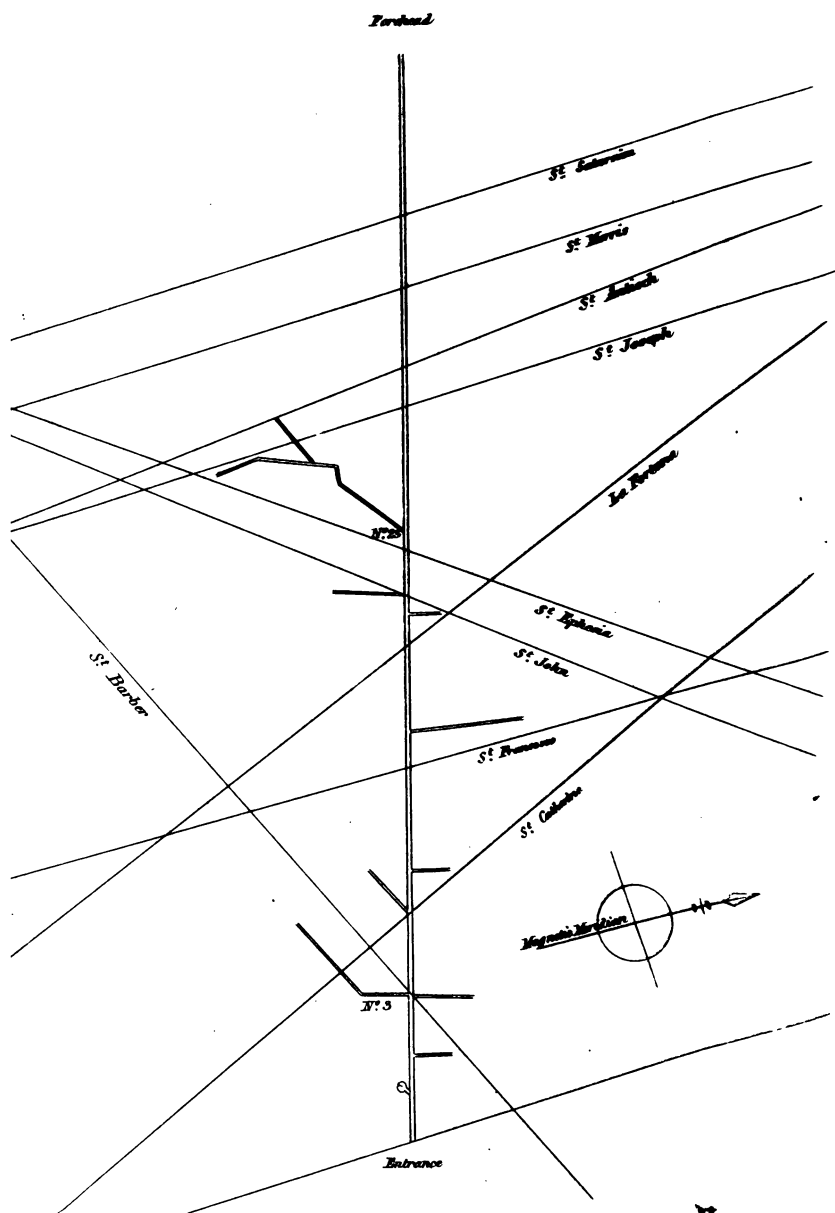
Mine Domenico Rosca. at Iglesias.

by the fusion of the natural sulphurets neither agrees with the composition of the sulphurets examined, nor with the acids of arsenic. From the theory of chemical proportions, it is obvious that it cannot be a simple sulphuret. It is probably the result of the combination of a higher degree of sulphuration with realgar. We know that arsenic acid is decomposed by sulphuretted hydrogen gas. The resulting sulphuret must be composed of 100 arsenic combined with 106·91 of sulphur. If we calculate the composition of a combination of this sulphuret with realgar in which the arsenic in each sulphuret is in equal quantity, 100 of arsenic in it will be combined with 74·6 of sulphur. If, on the contrary, we admit a composition such that the sulphur of the persulphuret is double that of the realgar, 100 of arsenic in it will be combined with 71·26 of sulphur, which agrees perfectly with the analysis of Laugier.

On examining the maximum of sulphuration of which arsenic is capable, I have found that this metal and sulphur may be mixed in almost all proportions. The sulphur for some time swims upon the surface of the fused sulphuret; but by degrees it mixes with it into a homogeneous yellow mass. I have in this way united arsenic with more than seven times its weight of sulphur. The sulphuret, when cool, was elastic, like caoutchouc, just as happens sometimes with sulphur itself, and some weeks elapsed before it became quite solid and hard. The quantity of sulphur was determined by dissolving it in nitromuriatic acid, and precipitating the sulphuric acid by muriate of barytes. When we distill this sulphuret, it gives at first sulphur, containing little arsenic; but as the process advances, the arsenic increases in quantity in the product distilled, which becomes at the same time more coloured; so that the last drops sublimed into the top of the retort have a fine ruby red. Hence it appears that heat does not furnish a method of obtaining the sulphurets of arsenic in the state of definite combinations.

(To be continued.)

Article X.

Memoir relative to the Lead Mines of Sardinia.
(With a Plate. See CIV.)

The mines in Sardinia have, there is no doubt, been wrought extensively at a very remote period. History relates, that both the Romans and Carthagenians carried on mining operations when they possessed the island. Zurita, in his history of the 14th century, as also the Genoese historians, Frederici and Giustiniano, state, that the silver which was on board the Pisan fleet, captured in 1283 by the Genoese, and which the latter

employed in defraying the expenses incurred in building the arsenal at Genoa, was the produce of the island of Sardinia.

· The great extent, and the irregular shape, of the excavations formed for the purposes of mining in Sardinia excite surprise; and the appearances of what seems to have been an unprofitable waste of money and labour give rise to doubts as to what has been the actual cause of these works of the early miners. To suppose that the enormous excavations which remain have at any time been filled with solid masses of ore is impossible; and it is equally difficult to believe that vast chambers, some of them formed in the shape of domes, and others as extensive square and circular apartments, should have been constructed without a view to gain. That these chasms are natural is an idea which cannot for a moment be entertained; and perhaps the most satisfactory way in which to account for their existence is to concur in a tradition prevalent in the island, that constructed at first for the purposes of the miner, these caverns served in after times as a residence for the natives, when domestic feuds of a political nature, or when hostile invasion, desolated the country. For these purposes it is conjectured that the mines have been gradually enlarged and altered till they assumed their present form; and that, like the Gothic inhabitants of Spain, who are known to have sought a similar species of shelter to escape the tyranny of their Moorish conquerors, the Sardinians were wont to seclude themselves in these subterraneous dwellings to preserve the freedom, the inheritance of their northern ancestors.

The entrances to these caverns are constructed upon several different plans; in some it is merely a horizontal creek in the rock; in others, it is wide enough to admit two coaches abreast; and in some it is a door of about seven feet six inches in height, and four feet three inches in breadth.

In most of these excavations, the ore appears to have been thoroughly wrought out, as hardly the least spark of it can be detected at present; and if the great number of mines in different districts of the island be considered, the wealth which the ancient possessors of the mines must have derived from them must have been immense.

The high grounds of Sardinia are composed of granite;* under this is a stratum of limestone, of the great thickness of 80 or 90 fathoms, of a compact and brittle texture, and of a whitish colour; and under this is generally a stratum of a brownish coloured schistus, frequently intermixed with some of a blue colour. In this limestone are situated the lead veins in a matrix

* The granite on the summit of the mountains here shows considerable appearances of stratification, which gives rise to a conjecture, that it rests in a stratum *above the limestone,* and does not penetrate through the strata of limestone and *schistus, and thus form* the base on which the last mentioned strata rest, according *to the received opinion* of geologists respecting the nature of granite mountains.

of barytes, calcareous spar, or quartz; and in the district called Cape of Cagliari, we can travel but a very short distance without crossing in our path many of these veins, the most of which bear a good rib of ore to this day. Their positions evidently indicate that they must form numerous intersections, and would afford profit if wrought with any tolerable degree of skill.

The mines at Iglesias are at present wrought by the Sardinian government: in the course of 80 fathoms, 11 different veins have been discovered, and the whole of these have afforded a large quantity of excellent ore in the course of the few partial trials which have been made.

The ravines which intersect Sardinia in all directions preclude the necessity which so often exists in England of spending years in dead ground, where the only profit to be expected is that of rendering the adjoining veins accessible, and where so great an outlay of money generally takes place. Wood can also in Sardinia generally be procured close to the mines free of expense; and these advantages are only in some degree counterbalanced by the unfavourable climate, the scarcity of water for washing the ore, and the barbarous state of the inhabitants of the country.

In driving the main level of the lead mine at Iglesias, so soon as a vein had been found, the plan had been to proceed immediately to excavate; and so long as some particular danger or inconvenience had not impeded the labour it had been continued. As the sides of the vein, however, were, in general, left untouched, this could not long continue; and when once abandoned, a new vein had been begun upon, and treated in the same manner. The irregularity of these workings is of course such as to preclude the possibility of making an instrumental survey of them. A written description, and a sketch of the mine Domenico Rosea near Iglesias is, however, annexed, which it is hoped will convey a tolerable idea of the workings.

When the position of the veins in this mine, and their proximity to each other, are considered, doubts cannot be entertained as to their forming many intersections, and thus affording the most favourable prospect to the miner. There is also a stream of water near the mine, and a convenient situation for a smelting house.

The mine of Monte D'Oro, near Iglesias, is also one of the most extensive and ancient in the island. Here is, perhaps, the best example of the mines having been used as a permanent residence: the entrance has been faced or built with finely cut stone, and within the mine, shafts or sinkings lead from one set of subterraneous galleries to those below. The workings are so intricate and extensive that the guides are obliged to place small sticks in the path by which they conduct the strangers to enable them to return in security. May not these have served for the dwellings of the exiled Romans when Sardinia was a place o

banishment for the Roman criminals? At the entrance of the mine are the ruins of a number of stone buildings.

The small city of Iglesias is finely situated at the base of a range of mountains, whose sides near the town are planted with gardens, and richly cultivated and ornamented with orange grounds. The houses of the town are but indifferent; but the streets are cleaner and better paved than those of Cagliari. When the Pisans wrought these mines, this was their station : the air is wholesome, and the town is well supplied with water from wells of six or seven fathoms deep : it may certainly be described as one of the most desirable places of residence in Sardinia. It contains about 1,500 inhabitants.

The hills in which the mine Domenico Rosea is situated is not extensive, being only one English mile and a half in length, and three quarters of a mile in breadth. The mountains in the neighbourhood bear so close a resemblance to it in structure and shape as to warrant the conjecture of their being equally productive in lead veins.

At Monte Bergani, about three miles to the east of Iglesias, and situated on the western declivity of the mountains, a trial has been made by means of an open cast, about four yards in length and one in breadth, and a beautiful vein of this breadth has been exposed. It has a rib of ore up its centre of about four inches wide, and the matrix is flesh-coloured barytes. This vein is studded with ore, and the rib increases in breadth downwards. The ore which was produced in the upper part of the vein was close-grained, and apparently rich in silver, but deeper ; it resembled the potter ore of England.

The roads in the vicinity of this rich vein are good, and there is a tolerable supply of wood and water in the neighbourhood.

A view of the strata in the neighbourhood of the road which leads from Iglesias to Flumini Majore is to be had during almost the whole distance ; a brown or bluish argillaceous schistus may be seen below, and the thick stratum of whitish limestone, before-mentioned, resting upon it.

At Flumini Majore the natives talk of the existence of an ore of silver, but the specimens of this metal occurring in the state of any of its ores in the island are now so scarce as to throw some discredit on the statement of Captain Belly and Count Vargus, both of whom represent even native silver as of common occurrence in the island.

Close to the village of Flumini Majore is the Vein Pietro di Fuoco. In it there is a rib of free potter ore $1\frac{1}{2}$ inch thick. This strong vein of flesh-coloured barytes stands in some places seven feet above the adjacent surface; the weather having acted upon the adjacent limestone rock must have washed it away, while the more insoluble barytes remained unaffected. The open *cast by which* the vein has been tried is about 10 yards in length, *two feet in* breadth, and seven feet deep. It bears 82° S.W.

At La Miza di li Ano Marudu is a vein of iron ore, of a fathom thickness ; some lead ore is mixed with it, and it bears 65° S.E.

A vein of calcareous spar containing copper ore about three feet wide, and bearing nearly north and south, occurs at Marcasita. The sides of this vein are coated with a yellow mineral earth.

The veins of St. Lucia and Johnny Longa in this neighbourhood are also very promising. The latter bears 83° N.E.

The village of Flumini Majore, near which the whole of the last mentioned veins are situated, is well supplied with wood and water ; the roads for some distance around it are, however, bad ; and, during the summer, the air is unwholesome, as the mountain streams in its vicinity are at that season nearly dried up, and form stagnant and noxious air in the plain surrounding the village. This small village, the houses of which are built of clay, is situated in a plain of about three miles in length, and one in breadth. High and rugged mountains surround it on all sides ; the inhabitants are either goat-herds, or earn their subsistence by cultivating the little valley in which they dwell. They are about 1000 in number, and although poor, they seem tolerably contented and happy.

Between Flumini Majore and Monte Vecchio, and distant about four miles from the latter place, after ascending a steep acclivity situated across the head of a valley, a vein of flesh-coloured barytes is crossed by the road, and near it are seen other veins which seem to have at some period been tried.

At Monte Vecchio, there is a vein of flesh-coloured barytes, which promises well, and which is at present wrought by a Signore Malacria, and a Neapolitan merchant, who pay a duty of one-twelfth per cent. to the government for their privilege. The mining here is conducted in the same unskilful way as at Iglesias, the vein is simply followed till water flows in upon them, and here their labour has been abandoned for this reason, with a rib of ore a yard wide extending before them.

This is one of the most ancient mines, and is supposed to have been a very profitable one. The present company at first drove a flank level to the vein, but as it ran downwards, they changed their plan, and adopted that of working by sinkings, which the flowing of the water also compelled them to relinquish. By beginning their level at the bottom of the hill all this inconvenience might have been avoided. The point of the vein is due east and west ; it declines about two feet in six, and the stratum is limestone similar to that of Iglesias.

At Monte Carna there is a promising vein of white barytes in a stratum of limestone.

At Maishtalesch near Pula, about 10 miles south-west of Cagliari, there is a vein in a ravine through which flows a small brook, in which a rib of solid ore about five inches wide may be seen. It is in the limestone stratum, and bears 32° S.E.

At Sa Gruttu Exeda there are some indications of copper, and at Monte Santa, about eight miles south-west of Cagliari, there is a promising lead vein.

Survey of the Mine Domenico Rosea, situated about two Miles to the N.W. of Iglesias.

Remarks left	Bear. and dist. 73, N.W.	Remarks right.
An opening	32	
	37	An opening.
An opening for a bed	40	
Ancient cross cut built up . .	58	
	70	Cross cut.
Cross cut	100	
Cross cut in St. Barber, vein	114	Ditto
	127	Ditto
Cross cut	146	Ditto
Cross cut St. Catharine	193	
	217	Ditto St. Catharine vein crosses here, and causes a rib often.
	260	Loose stones.
	305	St. Francesco, vein of soft mineral earth.
	314	Cross cut.
	336	Ditto.
Cross cut	350	Ditto.
La Fortuna	400	
	431	St. John vein.
St. John Vein	448	
	469	A vein crosses here.
St. Ephesia vein	486	Cross cut.
Two cross cuts >	500	Cross cut and rise.
	600	St. Joseph vein.
St. Antioch vein	633	Cross cut.
St. Morris vein.	652	
Ditto, ditto	692	
	760	St. Saturnim vein.
	800	No further accessible.

The level is supposed to extend about 80 links further.

No. 3, Cross cut.

25 S.W.	39 Links.
67 S.W.	88 Ditto.

Points of the west cross cut, No. 21.

52° S.W. 62 links. Has communication here with the old workings.

75° N.W. 10 links. Opens into the old workings on the right.

31° S.W. 62 links. At the end of the first 20 links of this
length, it opens into another cross cut,
bearing 72 S.W. 46 links.

4° S.E. 66 links. Forehead.

ARTICLE XI.

ANALYSES OF BOOKS.

*Reports on the Epidemic Cholera which has raged throughout
Hindostan and the Peninsula of India since August,* 1817.
(Published under the Authority of Government.) Bombay,
1819.

THIS publication presents us with a very interesting account
of the disease which has excited so much alarm, and committed
such ravages in India for some time past : five places had escaped
the malignant visitation ; and as it still continued its course una-
bated in different directions, as neither the rapidity of its
progress, nor the violence of its symptoms, seemed to be at all
modified by the state of the atmosphere in respect to heat or
cold, moisture or dryness, it is impossible to conjecture how far
it may yet extend its influence. It is known to have appeared
on board several ships, after their departure from India, on their
return voyage ; and it is, therefore, not improbable, that it may
find its way to Europe at no distant period. A similar disease
is described by Sydenham, as having prevailed in England in
1669, and 1674, 1675, 1676, though it was neither so extensively
propagated, nor of itself so fatal in its effects. Although, how-
ever the Indian epidemic was almost uniformly fatal, when left
to nature, it is consoling to observe, from these reports, that the
mortality was very trifling, when medical aid was had recourse
to at the commencement of the attack.

The following extract gives a curious history of its progress :

The cholera first appeared in August of last year (1817) in Zila Jessore,
situated about 100 miles north-east of Calcutta. There had been no previous
marked peculiarity in the weather. The preceding cold and hot months were no
wise different from those of former years; and the rainy season was proceeding
with its wonted regularity. To the authorities on the spot there, the disorder
seemed at first to be of a purely local description; and attributable to the intem-
perate use of rank fish, and bad rice. They were soon undeceived ; after nearly
depopulating the town of Jessore, it rapidly spread through the adjoining villages;
and ran from district to district until it brought the whole province of Bengal
under its influence. It next extended to Behar; and having visited the principal
cities west and east of the Ganges, reached the upper provinces. There its progress
was more irregular. Benares, Allahabad, Goruckpore, Lucknow, Cawnpore,
and the more populous towns in their vicinity, were affected nearly in the regular
course of time; but it was otherwise in more thinly peopled portions of the coun-
try. The disease would sometimes take a complete circle round a village, and
leaving it untouched, pass on, as if it were about wholly to depart from the dis-
trict. Then, after a lapse of weeks, or even months, it would suddenly return, and,
scarcely reappearing in the parts which had already undergone its ravages, would

nearly depopulate the spot, that had so lately congratulated itself on its escape. Sometimes after running a long course on one side of the Ganges, it would, as if arrested by some unknown agent, at once stop; and taking a rapid sweep across the river, lay all waste on the opposite bank. It rarely, however, failed to return to the tract, which it had previously left. After leaving a district or town, it sometimes revisited it, but in such cases the second attacks were milder; and more readily subdued by medicine than those in the primary visitation.

The disorder showed itself in Calcutta in the first week of September. Few were seized in the beginning; but of those few scarcely one survived. Each successive week added strength to the malady; and more extended influence to its operation. From January to the end of May it may be said to have been at its full height; and during the whole of that period, the deaths in the city seldom by the police returns fell short of 200 a week.

It in turn attacked every division, and almost every corps in the army. Of its fatal effects amongst the troops, a melancholy and signal instance is afforded in the history of its appearance in the centre division of the field army, under the personal command of the Most Noble the Commander in Chief. There it commenced its attack on the 18th or 19th of November; was at its utmost violence for four or five days; and finally withdrew in the first days of December. The division consisted of less than 10,000 fighting men: and the deaths within 12 days amounted at the very lowest estimate to 3,000; according to others, to 5,000, and even 8,000. * The average loss of rank and file was between 80 and 90 men a battalion.

The epidemic was long in crossing the Bundlekund and Rewa Hills. It began to show itself at Jubbulpore on the 10th of April; prevailed generally amidst the corps posted there at Mundelah, Saugor, and other subordinate stations, to the 21st; and nearly disappeared before the end of the month. Here its influence was singularly irregular. In the same camp, and under circumstances precisely similar; some corps were entirely exempt; others had a few mild cases only; and others again suffered very severely. The same irregularity held in different descriptions and classes of troops. The disease did not reach Colonel Adams's camp till the 29th of May. It raged very violently during four or five days, and continued its operations in a desultory manner till the middle of the succeeding month. In Bengal and the middle provinces, it may now, perhaps, be considered as nearly at an end. Cases no doubt still now and then occur in Calcutta and its vicinity; but these are rare, and should rather be reckoned sporadic, than as proofs of the subsistence of the epidemic. The returns from the different divisions of the army now leave the head of cholera morbus, in most cases, blank; and the reports of the civil surgeons are equally decisive of its general disappearance. At Delhi, Futtigur, and others of the more northern stations, whither the disease was long in spreading, it is still, the Board believe, in full force, and producing the most alarming mortality.

The epidemic continued its. course in the same irregular manner, and reached Bombay in August, 1818. It is evident from these reports, that the disease presented itself in an infinite variety of forms; still the characteristic symptoms were the same in all, however much their order might be changed. Among the natives, the rapid approach of debility was principally to be dreaded, as the powers of nature seemed at once to be destroyed by the visceral congestion. Of this form of the disease, the extract which follows gives an excellent description.

The attack was generally ushered in by sense of weakness, trembling, giddiness, nausea, violent retching, vomiting and purging of a watery, starchy, whey-coloured, or greenish fluid. These symptoms were accompanied, or quickly followed, by severe cramps; generally beginning in the fingers and toes, and thence extending to the wrists and fore arms, calves of the legs, thighs, abdomen, and lower part of the thorax. There were soon succeeded by pain, constriction, and oppression of stomach and pericardium; great sense of internal heat; inordinate thirst; and incessant calls for cold water, which was no sooner *swallowed* than rejected, together with a quantity of phlegm, or whitish fluid, like

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* *The latter* calculations must include the deaths among the followers of the *camp*.

seethings of oatmeal. The action of the heart and arteries now nearly ceased; the pulse either became altogether imperceptible at the wrists and temples; or so weak as to give to the finger only an indistinct feeling of fluttering. The respiration was laborious and hurried; sometimes with long and frequently broken inspirations. The skin grew cold, clammy, covered with large drops of sweat, dank and disagreeable to the feel, and discoloured of a bluish, purple, or livid hue. There was great and sudden prostration of strength, anguish, and agitation. The countenance became collapsed, the eyes suffused, fixed, and glassy, or heavy and dull, sunk in their sockets, and surrounded by dark circles, the cheeks and lips livid and bloodless, and the whole surface of the body nearly devoid of feeling. In feeble habits, where the attack was exceedingly violent, and unresisted by medicine, the scene was soon closed. The circulation and animal heat never returned; the vomiting and purging continued, with thirst and restlessness; the patient became delirious or insensible, with his eyes fixed in a vacant stare, and sunk down in the bed; the spasms increased generally within four or five hours.

The disease sometimes at once, and as if it were momentarily, seized persons in perfect health; at other times those who had been debilitated by previous bodily ailment; and individuals in the latter predicament generally sunk under the attack. Sometimes the stomach and bowels were disordered for some days before the attack, which would then in a moment come on in full force, and speedily reduce the patients to extremities.

Such was the general appearance of the disease where it cut off the patient in its earlier stages. The primary symptoms, however, in many cases, admitted of considerable variety. Sometimes the sickness and looseness were preceded by spasms. Sometimes the patient sunk at once after passing off a small quantity of colourless fluid by vomiting and stool. The matter vomited in the early stages was in most cases colourless or milky; sometimes it was green. In like manner, the dejections were usually watery and muddy; sometimes red and bloody; and in a few cases they consisted of a greenish pulp, like half digested vegetables. In no instance was feculent matter passed in the commencement of the disease. The cramps usually began in the extremities; and thence gradually crept to the trunk; sometimes they were simultaneous in both; and sometimes the order of succession was reversed; the abdomen being first affected, and then the hands and feet. These spasms hardly amounted to general convulsion. They seemed rather affections of individual muscles, and of particular sets of fibres of those muscles: causing thrilling and quivering in the affected parts like the flesh of crimped salmon; and firmly stiffening and contorting the toes and fingers. The patient always complained of pain across the belly; which was generally painful to the touch, and sometimes hard and drawn back towards the spine. The burning sensation in the stomach and bowels was always present; and at times extended along the cardia and œsophagus to the throat. The powers of voluntary motion were in every instance impaired, and the mind obscured. The patient staggered like a drunken man, or fell down like a helpless child. Headach over one or both eyes sometimes, but rarely, occurred. The pulse, when to be felt, was generally regular, and extremely feeble, sometimes soft, not very quick, usually ranging from 80 to 100. In a few instances, it rose to 140 or 150, shortly before death. Then it was distinct, small, feeble, and irregular; sometimes very rapid; then slow for one or two beats. The mouth was hot and dry; the tongue parched and deeply furred, white, yellow, red, or brown. The urine at first generally limpid, and freely passed; sometimes scanty, with such difficulty as almost to amount to strangury; and sometimes hardly secreted in any quantity; as if the kidneys had ceased to perform their office. In a few cases the hands were tremulous. In others the patient declared himself free from pain and uneasiness; when want of pulse, cold skin, and anxiety of features, portended speedy death. The cramp was invariably increased upon moving.

Where the strength of the patient's constitution, or of the curative means administered, were, although inadequate wholly to subdue the disease, sufficient to resist the violence of its onset; nature made various efforts to rally, and held out strong but fallacious promises of returning health. In such cases, the heat was sometimes wholly, at others partially, restored; the chest and abdomen in the latter case becoming warm, whilst the limbs kept deadly cold. The pulse would return, grow moderate and full, the vomiting and cramps disappear, the nausea diminish, and the stools become green, pitchy, and even feculent; and with all these favourable appearances, the patient would suddenly relapse; chills, hiccup, want of sleep,

and anxiety would arise; the vomiting, oppression, and insensibility return, and in a few hours terminate in death.

When the disorder ran its full course, the following appearances presented themselves: What may be termed the cold stage, or the state of collapse, usually lasted from 24 to 48 hours, and was seldom of more than three complete days' duration. Throughout the first 24 hours, nearly all the symptoms of deadly oppression, the cold skin, feeble pulse, vomiting and purging, cramps, thirst and anguish, continued undiminished. When the system showed symptoms of revival, the vital powers began to rally, the circulation and heat to be restored, and the spasms and sickness to be considerably diminished. The warmth gradually returned, the pulse rose in strength and fulness, and then became sharp and sometimes hard. The tongue grew more deeply furred, the thirst continued, with less nausea. The stools were no longer like water, they became first brown and watery, then dark, black, and pitchy; and the bowels, during many days, continued to discharge immense loads of vitiated bile, until, with returning health, the secretions of the liver and other viscera gradually put on a natural appearance. The fever, which invariably attended this second stage of the disease, may be considered to have been rather the result of nature's effort to recover herself from the rude shock which she had sustained, than as forming any integrant and necessary part of the disorder itself. It partook much of the nature of the common bilious attacks prevalent in these latitudes. There was the hot dry skin; foul, deeply furred, dry, tongue; parched mouth; sick stomach; depraved secretions; and quick variable pulse; sometimes with stupor, delirium, and other marked affections of the brain. When the disorder proved fatal after reaching this stage, the tongue, from being cream-coloured, grew brown, and sometimes dark; hard, and more deeply furred; the teeth and lips were covered with sordes; the state of the skin varied; chills alternating with flushes of heat; the pulse became weak and tremulous; catching of the breath; great restlessness and deep moaning succeeded; and the patient soon sunk insensible under the debilitating effects of frequent dark, pitchy, alvine, discharges.

Among the Europeans, as might be expected, the disease assumed a character somewhat different from the above. According to Mr. Crow, it was in them more nearly allied to tetanus than to cholera. This distinction was of great use as indicating the mode of cure. The following extract is from Mr. Crow's report:

In these corps the disease makes its appearance sometimes by the same affection of the stomach and bowels as in the natives, frequently with spasm in the feet, legs, abdominal muscles or arms; but in all, the spasmodic affection is the pre-eminent one, head-ache, pain in the eyes, excruciating pain at the scrobiculus cordis (a pathognomic symptom of tetanus) quick, full, hard pulse (but labouring and oppressed according to the violence of the spasms), retention or difficulty of voiding the urine, strong and violent spasm drawing up the legs, rigidly contracting the arms and fingers, bending the body forwards, or backwards, or laterally, the patient at the same time exerting such physical strength as requires half a dozen of men to hold him on his cot. I have already said that the intestinal evacuations are watery and clay-coloured: this must not be lost sight of, as indicating a want of bile; the vomitings are somewhat of the same kind, attended with eructations, while the bowels are distended with flatus. These combined with a very distressing tenesmus, not to be allayed by anodyne enemas, strongly point out that nature requires relief by the bowels. After the second day that the disease made its appearance in the 65th, Dr. Burrell commenced blood letting with the most decided advantage. This has, therefore, become the first grand remedy amongst the Europeans, and in which he has been followed by the practitioners in other European corps, and with the same result. Bleeding quoad vires, the calomel and opiate, the hot bath, warm clothing, and frictions spirituous, or anodyne, form the chain of treatment in the European hospitals here; and these are repeated again and again as the symptoms may seem to demand. Under this system, and *early* application for relief, I think the disease is not fatal in a greater proportion than 1 in 100 cases.

A fact is recorded in this volume highly honourable to the

Bombay government. When the epidemic approached that presidency, Sir Evan Nepean authorized the Medical Board to take whatever steps they thought as most likely to check its progress, or to alleviate the calamities which it might be expected to produce in so populous a place as Bombay. In consequence of this permission, as it was evidently impossible from the prejudices of the natives to collect them together in hospitals, a number of native assistants were hired and stationed in different parts of the island, in order to afford medical aid at the houses of those who might require it. For their instruction and guidance, a brief description of the disease, and of the mode of cure, was drawn up, and translated into the different languages which they understood. This most difficult and most important part of the arrangement was principally conducted by Dr. Taylor, from whose report we shall extract an account of the mode of treatment which was found most successful, with which we shall close our observations on this publication.

The method of cure which, after consulting with you, I ordered to be used by the native assistants, was extremely simple. They were supplied with scruple doses of calomel, and a mixture composed of laudanum, essence of peppermint, brandy, and water, each ounce of which contained 50 minims of laudanum, 10 minims of essence of peppermint, three drachms of brandy, and four drachms of water. The calomel was first given in powder on the tongue, and then washed down with an ounce of the mixture. A similar dose was ordered to be repeated in two or three hours, if the patient derived no material relief from the former, or to be repeated immediately should the first be rejected, a circumstance, however, which very seldom happened. Besides giving these medicines, the assistants were directed, in all cases where it was practicable, to use the warm bath; and when, as it generally happened, this could not be done, to endeavour to alleviate the spasms, and the pain in the abdomen by fomentations with cloth wrung out of warm water, or by fomentations with warm bricks or tiles, or salt wrapped up in cloths. Frictions with warm spirits were also directed, which almost uniformly afforded great relief. The patients were ordered to be laid on a cot, underneath which, shigras, filled with warm ashes, were placed when it was necessary; vessels filled with warm water were also applied to the extremities. When by the use of these remedies the more violent symptoms were removed, but some pain or uneasiness in the abdomen still continued, and the bowels were not moved, an ounce, or an ounce and a half, of castor oil was given. In addition to the other stimulants already mentioned, I sometimes directed cloves and cardamoms to be taken, when the extremities were cold and the pulse feeble. Particular injunctions were given not to allow the patient to drink cold water; but to allay in some measure his urgent thirst, he was permitted sparingly the use of warm congee. The assistants were also enjoined not to suffer any one to be disturbed who felt a disposition to sleep.

As the majority of cases were seen only by the native assistants, I have judged it proper to give this account of the general plan of practice they were directed to pursue. Considering every circumstance, the success attending it has been much much greater than could have been expected.

The same practice was adopted by myself, with this exception, that usually I had recourse in the first place to bleeding. The accounts I had read of the disease, and of some dissections which showed a great congestion of blood in the abdominal and thoracic vessels, led me to conclude, that bleeding, in many cases, would be the most efficacious remedy. Accordingly it will be observed that I wished to try the effect of blood letting in one of the first cases, but was prevented by the unwillingness of the patient. A day or two afterwards I was called to see a person who had been ill 18 hours, and had received from one of my assistants two doses of calomel and two laudanum draughts. At the time I saw him, though his mouth was affected, he had excruciating burning pain in the abdomen, with tormenting thirst

and spasms. With some difficulty I prevailed on him to submit to bleeding, and took from him at least 24 ounces. During the bleeding, the pain in the abdomen entirely ceased; and what is a little singular, on his arm being tied up, he lay down on his left side, which the people of the house said he had not been able to do before, though he did not complain of any uneasiness in the region of the liver. As slight spasms still continued, I ordered him to be put into the warm bath. By these means, and the exhibition afterwards of a dose of castor oil, he entirely recovered.

From this time, bleeding was very generally adopted in the cases which I had an opportunity of seeing: latterly also it was had recourse to by such of the assistants as had learned to bleed, and was sometimes even urged by the patients themselves and their friends. In almost every case it relieved the pain in the abdomen and the spasms, and when the principal symptoms were great oppression at the breast, laborious breathing, and a sense of suffocation, or when the patient had trismus, or general tremors with giddiness, bleeding was the only remedy which afforded effectual relief.

When it could be obtained, the usual quantity of blood taken away was 24 ounces, and no case occurred to me of the disease after such copious bleeding (for in a native it may be called copious) proving fatal. In two or three instances, however, it was found expedient to repeat the bleeding.

But while bleeding in an early stage of the disease, and under certain circumstances, almost uniformly produced the most decided and salutary effects, it was in general unavailing in the latter stages, or in the worst forms of the disease, when the extremities were cold, the pulse could not be felt, and the eyes fixed and sunk. In such cases indeed it was impossible, as has been already observed, to procure a proper discharge of blood, which merely trickled down in small drops; and opening the temporal artery was attended with no advantage, for by this means I never obtained more than two or three ounces of blood. Under such circumstances, no pulsation could be felt in the artery, and, except in one or two instances, the blood flowed out of it without any pulsatory motion. Almost the whole of these cases proved fatal. A few, however, in which the discharge of blood, though small, was followed by faintness and profuse perspiration, terminated favourably.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

March 23.—A paper, by Mr. J. Hood, was read, entitled, " On the Means of supplying Muscles in a State of Paralysis with nervous Power." The author having remarked the effects of nitrate of silver in removing the spasmodic action of the urethra, when applied to a stricture near its orifice, concluded that this salt has the property of influencing the action of the nerves at a considerable distance from the place where applied. Observing likewise the slight discharge produced by an eschar made by the nitrate of silver, he was induced to ascribe to it the power of exciting the absorbents to vigorous action by nervous communication, and in this manner he explained the good effects of the remedy in question in a case of diseased knee joint, when applied so as to produce an eschar. Other cases were related in which the external application of nitrate of silver proved stimulating to the nervous system without proportionally increasing the action of the vascular system. Hence the author concluded that muscular spasm and paralysis are caused by

diminished nervous action; that muscular spasm cannot exist where the temperature is steadily above 90°, and that animal heat is produced principally by the action of the brain and nerves. Nitrate of silver, according to the author, applied to the head or spine, elevates the temperature, subdues spasm, and restores strength in certain paralytical cases; and applied to enlarged joints, produces a more rapid absorption than any other remedy.

The Society adjourned till after Easter.

April 13.—The Society resumed its meetings, and a paper, by Sir E. Home, was read on the Milk Teeth, and Organs of Hearing of the Dugong. The skull from which the following description was taken, and which is the only perfect one in Europe, was sent from Sumatra by Sir Stamford Raffles. The milk tusks of this animal resemble those of the narwhale and elephant, being, like them, deficient in external smoothness, when compared with the permanent tusks. But they are peculiar in havin a shallow cup attached to their base, apparently for the purpose of receiving the point of the permanent tusks as soon as formed, and for directing them forward in the same course as that of the milk tusks, and which is different from that in which the permanent tusks were originally directed. The milk tusks of the dugong have hitherto been mistaken for its permanent tusks; but as no full grown individual has been yet examined, the form, &c. of the permanent tusks are unknown.

The grinding teeth of this animal differ from those of all others. They consist of a double cone, the external crust of which is not enamel. This crust covers an internal harder coat, and the bulk of the tooth consists of soft ivory; hence in wearing down, they will assume a concave form.

The organs of hearing also in this animal are quite peculiar. The malleus and incus are fastened to the sides of the tympanum by a bony substance extending across the intervening space. The stapes is opposed to, but not connected with, the foramen ovale, nor is it anchylosed with the ramus of the incus. The handle of the malleus projects in the centre of the circle over which the membranum tympani had been spread; and hence, in the recent animal, is probably attached to the centre of that membrane As the habits of the dugong resemble those of the hippopotamus, Sir Everard was induced to examine the organs of hearing in the latter animal to see if they were similar to those of the dugong. He found them, however, very different, the ossicula auditus being detached from the skull, and readily dropping out at the external orifice. In the dugong, the semicircular canals and cochlea are very small. Sir Everard was induced to conclude from the above remarkable construction of the organs of hearing, that this animal, perhaps more than any other, hears by means of vibrations conveyed through the bones of the skull to the canals and cochlea.

ROYAL ACADEMY OF SCIENCES AT PARIS.

An Analysis of the Labours of the Royal Academy of Sciences
of Paris during the Year 1818.

(*Continued from* p. 305.)

PHYSICAL SCIENCES.—By M. Le Chevalier Cuvier, Perpetual
Secretary.

CHEMISTRY.

Chemistry has been enriched this year with two new substances
which are doubly interesting; because one is a substance not
only metallic, but also alkaline; that is to say, its oxide is a new
fixed alkali; and the other is metallic, acidifiable, and more ana-
logous to sulphur than to any other substance.

We owe the first to M. Arfvedson, a young Swedish chemist,
a pupil of Prof. Berzelius. M. Arfvedson discovered it in a
stone called *petalite,* in which he did not find more than from
three to five per cent. of it; but he afterwards found as much as
eight per cent. of it in another stone called *triphane.*

This substance affords very fusible salts, with the greatest part
of the acids; its carbonate, when melted, attacks platinum nearly
as powerfully as the nitrates of the other alkalies, and is difficultly
soluble; its muriate is very deliquescent; its sulphate crystallizes
without any water of crystallization.   The capacity of this alkali
for saturating acids is much greater than that of any other alkali,
and it also enters into the salts which it forms with the acids in a
much greater proportion.

The author of this discovery has given the name of *lithion* to
this new substance, in order that we may recollect that this alkali
was discovered in a mineral; whereas the other two fixed alka-
lies were originally extracted from vegetables.

The second new substance was discovered by Prof. Berzelius
himself in a manufactory of oil of vitriol, at Fahlun, in Sweden.
There is deposited on the floor of the chamber where the sulphur
(distilled from pyrites) is burned, a reddish mass, which, for the
most part, consists of sulphur; but on being set on fire, it exhales
a very strong odour of horseradish.   Now this smell being one
of the characters belonging to a metal discovered a few years
ago by M. Klaproth, and called *tellurium,* it was suspected that
this smell was owing to a mixture of this metal with the sulphur.
Nevertheless Messrs. Berzelius and Gahn, who first examined
this red substance, were not able to extract any tellurium from
it.   The first mentioned gentleman carried some to Stockholm,
in order to examine it more closely, and found in it a very vola-
tile substance, very easily reducible, and which was not precipi-
table by alkalies.   Its colour is grey, very shining, it is hard,
friable, and its grain resembles that of sulphur.   Its specific

gravity is 3·6. It produces a red powder by trituration, is softened at the temperature of boiling water, a little above which it melts; and, while cooling, it remains for some time soft, plastic, and capable of being drawn out into threads in the same manner as sealing wax. At a temperature a few degrees higher, it boils, and sublimes in the form of a yellowish gas, and condenses into beautiful red flowers, without undergoing oxidation. It evaporates in the open air in a red smoke, and burns with a blue flame, exhaling so strong a smell of horseradish that the thirtieth part of a grain is sufficient to infect the air of the largest room.

Prof. Berzelius has given the name of *selenium*, derived from the Greek name of the moon, to this substance, that we may recollect the affinity it has with tellurium; an affinity which may, perhaps, only arise from the presence of selenium itself in every specimen of tellurium hitherto examined.

These discoveries having been announced to the Academy by M. Gillet-Laumont, and soon afterwards confirmed by a letter of Prof. Berzelius written to M. Berthollet, M. Vauquelin immediately set about verifying the report with respect to the alkali; and his observations have added some details to those which M. Arfvedson had given. Although M. Vauquelin had only a small quantity of petalite at his command, he found in it as much as seven per cent. of lithion.

Prof. Berzelius has followed up his discovery of selenium with the great care that it merited. He has treated it with most of the chemical agents, and examined their actions upon it; and, having come to Paris this year, he has given a very detailed account of his labours in the Annales de Chimie. He shows that, taking every circumstance into consideration, selenium is an intermediate substance between the combustible and metallic substances.

He has exhibited comparisons between selenium, with sulphur and tellurium on the one side, and with chlorine, fluorine, and iodine, on the other: all of them substances which many chemists have lately wished to class along with sulphur, because they yield, like that substance, acids, by being combined with hydrogen. What we have said on this subject in the analyses of 1813 and 1814, in giving an account of the new theory of Sir H. Davy, respecting those acids which he considers as being formed without oxygen, may be recollected.

M. Berzelius finding the combinations of sulphur, tellurium, and selenium, with metals and combustible substances, to have a great analogy to one another, and on the other side, that the combinations of iodine and chlorine, with the same substances, have also a great analogy betwixt themselves and with those of oxygenized acids, though they do not resemble in the least the preceding, concludes from hence, that they constitute two very distinct orders of substances; and by this he shows

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very plainly that he does not consider the theory of Sir H. Davy as being demonstrated.

Selenium is excessively rare; 500 lbs. of sulphur, when burned in the manufactory of Fahlun, yields only one-third of a gramme. In how much smaller proportion then must it be in the pyrites from which the sulphur is extracted! Prof. Berzelius has since found it forming about the one-fourth part of an extremely rare ore of silver and copper, extracted from a mine now abandoned in the province of Smoland, in Sweden, which he had considered, on account of its smell, as an ore of tellurium. He has also found some specimens of it combined with copper without any silver.

The more we reflect on these chemical elements, which appear to be scattered at random by nature in such minute quantities, that the most delicate exertions of art, and the most profound science, are required to discover them, the more we are led to believe that still more profound researches will hereafter strip them of their rank of elements.

M. Gay-Lussac made some researches in 1811 upon the colouring principle of prussian blue, or that substance which has been called for some time the *prussic acid*. These researches showed that this substance, in a state of purity, had very remarkable properties, of which we were until then entirely ignorant; such as, among others, the very small interval between its freezing point and that of its evaporation, and its dreadful power upon the animal economy. This experienced chemist, continuing his researches upon this important subject, discovered in 1814 that this principle was a hydro-acid; that is to say, one of those substances which resemble acids in their action upon other bodies, but in which the presence of oxygen could not be demonstrated, and which appear to be formed by the combination of hydrogen with a radical. The prussic acid is really the first hydro-acid whose radical is known in respect to its elements, as M. Gay-Lussac found that it was composed of carbon and azote in slightly different proportions. He called the radical *cyanogen*, and the acid produced from it hydrocyanic acid, on account of its property of giving a blue colour to oxide of iron. We announced all these discoveries in our Analyses for 1811 and 1814.

M. Vauquelin has turned his attention to this subject, following, as, with his accustomed modesty, he expresses himself, the road which M. Gay-Lussac had marked out for him: nevertheless this road had some branches which could not escape a man like M. Vauquelin.

Gaseous cyanogen is absorbed by about four times and a half its bulk of water, and communicates a very sharp taste and smell to it, but without colouring it. This solution, in the course of some days, becomes yellow, and afterwards brown; it deposits a brown matter, acquires the odour of hydrocyanic acid, and on the addition of potash, ammonia is developed. Nevertheless

it will not yet form prussian blue. Further experiments showed that it contained hydrocyanate and carbonate of ammonia, and also ammonia combined with a third acid which M. Vauquelin calls the cyanic, but without absolutely determining the composition of its radical.

The water, therefore, is decomposed; part of its hydrogen combines with one part of the cyanogen, and forms hydrocyanic acid; another part unites with the azote of the cyanogen, and forms ammonia; the oxygen of the water forms carbonic acid with one part of the carbon of the cyanogen. The third acid results from some combination of the same kind, and there still remains some carbon and azote which could not be converted into any of those acids from a deficiency of oxygen, and which produces the brown deposit.

Alkaline oxides produce similar effects, but much more quickly.

Cyanogen, treated with a number of other oxides, metals, and combustible substances, afforded results not less curious to M. Vauquelin. The most interesting question that could be resolved was the inquiry whether prussian blue is a cyanuret or a hydrocyanate; that is to say, whether it is a combination of oxide of iron with cyanogen, or rather with its hydro-acid. M. Vauquelin having found that water impregnated with cyanogen can dissolve iron without changing it into prussian blue, and without the disengagement of any hydrogen gas, and that prussian blue was left in the undissolved portion; while hydrocyanic acid converts iron or its oxide into prussian blue without the help either of alkalies or of acids; he has concluded from hence, against the opinion of M. Gay-Lussac, that prussian blue is a hydrocyanate, and that when iron is exposed to water impregnated with cyanogen, there is not only formed in it cyanic acid, which dissolves a part of the iron, but also, and at the same time, hydrocyanic acid, which changes another part of the iron into prussian blue.

He even establishes it as a general rule, that those metals which, like iron, decompose water at the ordinary temperature of the atmosphere, form hydrocyanates; and that those metals which do not possess this power, as silver and quicksilver, form only cyanurets.

It is well known that most acids are formed by the combination of oxygen with certain substances to which the name of radicals is given, and that the acid thus formed differs in its properties according as there enters into the combination a greater or less proportion of oxygen, and is called by a different name, to which modern chemists have given a certain regularity, indicating the degree of oxidizement by means of the termination.

It is thus that azote produces, by successive additions of oxygen, nitrous gas, nitrous acid, nitric acid; and we have mentioned in our Analyses for 1816 other combinations, which differ in their proportions, discovered by Messrs. Gay-Lussac and Dulong.

M. Thenard has lately made some experiments, from which it appears that many acids will admit the combination of much larger proportions of oxygen than those which have hitherto been regarded as their most highly oxygenized state. By carefully dissolving super-oxidized barytes in nitric acid, and precipitating the barytes from it by sulphuric acid, the excess of oxygen remains united with the former acid, which, by this means, becomes oxygenized nitric acid. It may be concentrated by the means pointed out by M. Thenard, to such a degree that it will yield by heat 11 times its bulk of oxygen, and is then, according to the calculation of this experienced chemist, a combination of one volume of azote with three volumes of oxygen. The hydrochloric acid is oxygenized by the same means, and acquires some singular properties; for on being applied to oxide of silver, water and a chloruret are formed, and the disengaged oxygen produces an effervescence as violent as when an acid is poured upon an alkaline carbonate.

Sulphuric acid and fluoric acid may be oxygenized in the same manner, and all these acids may be again superoxygenized once or even oftener. M. Thenard has added in this manner to some as many as seven, and even 15 successive doses of oxygen. He has also forced hydrochloric acid to absorb a quantity of oxygen equal to 30 times its bulk. Nothing could equal the effervescence that then took place on its coming in contact with oxide of silver. The earths and metallic oxides may be also superoxygenized by means of the acids thus surcharged with oxygen, and by similar processes. M. Thenard has even superoxygenized water by pouring barytes water into superoxygenized sulphuric acid; the sulphuric acid united with the barytes, and ceded its excess of oxygen to the water. Water thus oxygenized freezes or evaporates in vacuo without losing its oxygen; on the contrary, it becomes more concentrated, until it has absorbed from 40 to 50 times its volume of oxygen; but boiling carries off the oxygen; charcoal, silver, the oxide of silver, and those of several other metals, occasion it to be thrown off with a violent effervescence; and it is very singular that this rapid change of a considerable quantity of matter into a gaseous state, so far from producing any cold, heats the liquor very sensibly. M. Thenard supposes that electricity has some share in this phenomenon.

It is known at present by the celebrated galvanic experiments of Sir Humphry Davy, that fixed alkalies are merely the oxides of extremely combustible metals; and by those of MM. Thenard and Gay-Lussac, that they can be reduced to the metallic state by means of charcoal and a very high temperature. We mentioned these grand discoveries in our Analysis for 1808.

M. Vauquelin, having lately reduced antimony by alkaline fluxes, perceived that this metal put into water yielded a great quantity of hydrogen gas, and that the water became alkaline.

Other metals, reduced in the same manner, produced the same appearances. From this he concludes, that a part of the alkali which he used combined in a metallic form during the operation with the antimony, and decomposed the water to return to the state of oxide; but he has of course been also obliged to conclude, that the presence of a metal is favourable to the reduction of the alkali, as otherwise it would not have taken the metallic form in so low a temperature.

We mentioned last year some experiments of MM. Chevillot and Edouard upon that singular combination of manganese and potash which is called the mineral cameleon, upon account of the facility with which its colour may be changed several times successively.

These young chemists have continued their labours, and have discovered that soda, barytes, and strontian, will yield different sorts of cameleons, by uniting, like potash, with the oxide of manganese and absorbing the oxygen from it; but, confining their principal attention to the species of cameleon formed by potash, in which the alkali is perfectly neutral, and which is of a fine red colour, they have found that those bodies which are very combustible act very energetically upon it, that they decompose it, and frequently take fire together with a strong detonation: phosphorus even produces a detonation by simple contact. On the other hand, this red cameleon, exposed to fire, is decomposed, and yields oxygen, black oxide of manganese, and green cameleon, in which potash is in excess.

They conclude from these facts, that in the formation of the cameleon, the result of the intervention of the oxygen is a further oxidizement of the manganese, and the conversion of it into a true acid; so that the cameleon is a manganesiate of potash. The red cameleon in particular, being a perfectly neutral manganesiate, and the green, a manganesiate with an excess of alkali. Still, however, they have not been able to insulate the acid whose existence they admit; but they have made numerous experiments which they imagine to confirm the opinion they published last year, that the green cameleon only differed from the red by having more alkali in its composition.

When acids are poured upon the green cameleon, or an alkali upon the red, they are equally changed from one colour to the other; even boiling and agitation are sufficient to disengage the excess of potash in the green cameleon, and to change it into red. Many acids also, when used in excess, decompose the cameleon entirely, by taking the potash from it, disengaging the oxygen, and precipitating the manganese in the state of black oxide. Sugar, gums, and several other substances, capable of taking away the oxygen, also decompose the cameleon; and an exposure to the air produces a similar effect, which these authors ascribe to the foreign particles floating in the atmosphere, and

which, falling into the solution, take away a portion of the oxygen that is essential to its existence.

Cobalt and nickel are two semimetals which it is very difficult to obtain in a state of purity, and still more difficult to separate entirely from one another; nevertheless this purification is necessary for an exact determination of their respective properties. M. Laugier having tried the newest methods that have been published as proper to obtain this object, has found in nickel unequivocal traces of cobalt. In order to get rid of them, he dissolved the mixed metal in ammonia, and precipitated it by oxalic acid; he then redissolved the oxalate of nickel and cobalt obtained by this operation in concentrated ammonia, and exposed the solution to the air. As the ammonia exhaled, oxalate of nickel mixed with ammonia was deposited. The nickel was entirely separated from the liquid by repeated crystallizations; so that there remained only a combination of oxalate of cobalt and ammonia, which was easily reduced. The small quantity of cobalt that remained in the nickel that was precipitated was separated by several successive solutions in ammonia; so that one and the same operation yielded both metals in a state of purity.

Sugar of milk, treated with nitric acid, yields an acid, originally discovered by Scheele, and which has been since called the mucic acid, because it is also produced by the action of nitric acid upon gums and mucilages. When this acid is exposed to heat, a brown saline matter is sublimed, of a strong smell, soluble in water and alcohol, and burning with a flame upon lighted charcoal. Trommsdorf, who particularly examined this sublimed substance, conceived he had found in it succinic acid, pyrotartaric and acetic acids, and several other substances; but M. Houton-Labillardiere perceiving, upon the reading of Trommsdorf's essay, that he attributed to the succinic acid very different characters from those that this acid really possesses, thought proper to resume the research.

He has read a memoir to the Academy, in which he proves that this pretended succinic acid is a new acid, to which he gives the name of pyromucic acid. When it is cleared from the oil and acetic acid with which it is mixed, it easily crystallizes, and is white, scentless, of a strong acid taste; it melts at 130 centigrade degrees (266° Fahr.) is volatilized at a little above that temperature, does not attract moisture, dissolves much more abundantly in boiling water than in cold; and upon its being resolved into its constituent parts, there were obtained from it about nine volumes of the vapour of carbon, three of hydrogen, and two of oxygen. M. Houton-Labillardiere carefully describes the combinations of this acid with different salifiable bases, and all the appearances that he relates support the assertion of this *young* and skilful chemist.

M. Chevreul has made new and important additions to his researches upon fatty bodies, with which we have already several times entertained our readers. Having found that the matter of biliary calculi, which he calls *cholesterine*, does not form a soap with the alkalies—a circumstance which distinguishes it completely from fats ; he thought he had also found, that spermaceti, to which he gave the name of *cètine*, was reduced by the action of alkalies into an acid analogous to one of those two acids that the alkalies produced from fats; namely, into that which he called *margaric acid*, but that the acid of spermaceti had a much smaller capacity of saturation. He, therefore, judged it necessary to give this acid a peculiar name, and called it *cetic acid*. A continuation of these experiments has, however, convinced him that this acid is in fact only margaric acid, whose properties have been altered by some remains of a fatty matter not of an acid nature. But dolphin oil, when treated by M. Chevreul's method; that is to say, converted into soap by means of the alkalies, afforded, besides the two acids yielded by fatty substances, a third sort of acid, which he calls the *delphinic*, but which is not yielded by common fish-oil.

It must be noticed that oxygen is not found to be contained in these new ternary acids prepared from fats, and that they are, in respect to the common vegetable acids, the acetic acid, the oxalic, &c. the same as in the mineral kingdom, the hydro-acids of Sir H. Davy are, in respect to the old and well-known mineral acids, the nitric acid, the sulphuric, &c.

Cochineal, that singular insect, which, on account of the colouring matter that it yields, is become such an important article in commerce, not having been studied as yet by chemists with that attention which it deserved, MM. Pelletier and Caventou have made it the object of their experiments. They have found that the very remarkable colouring matter which composes the principal part of it, is mixed with a peculiar animal matter, a fat like common fat, and with different sorts of salts. The fat having been separated by ether, and the residuum treated with boiling alcohol, they either allowed the alcohol to cool, or gently evaporated it, and by this means they obtained the colouring matter, but still mixed with a little fat and animal matter ; these were separated from it by again dissolving it in cold alcohol, which left the animal matter untouched, and by mixing the solution with ether, and thus precipitating the colouring matter in a state of great purity. It is well known that this colouring matter is of a most beautiful red colour, and the chemists of whom we are speaking, give it the name of *carmine* (carminium). It melts at 50° (122° Fahr.) becomes puffy, and is decomposed, but does not yield ammonia. It is very soluble in water, slightly in alcohol, and not at all in ether, unless by the intermediation of fat. Acids change it from crimson,

first to bright red, and then to yellow : alkalies, and generally speaking all protoxides, turn it violet ; alumine takes it from water.

These experiments explain many of the processes in the art of dyeing and colour making, and particularly they explain what happens in dyeing scarlet, and in the manufacture of carmine and lake.

Lake is composed of carminium and alumine : it has the proper colour of carminium ; that is to say, crimson. Carmine itself is a triple compound of an animal matter, carminium, and an acid which enlivens the colour ; the action of muriatic acid in changing the crimson colour of cochineal into a fine scarlet is similar.

METEOROLOGY.

The most apparent causes of atmospheric phenomena, such as the density of the air, its moisture, its heat, and its electricity, appear to depend principally upon the action of the sun : nevertheless the irregularity of their effects in our climates are sufficient to show that there exists influences of a different kind, and that they are complicated with causes still unknown : it is this complication which renders meteorology, even at present, the branch of the physical sciences which has made the smallest approach to that degree of certainty which is necessary to its being considered as a real science.

M. Humboldt remarks, that if any hope exists that the laws of meteorology can ever be discovered, it must be by studying it in those climates where the phenomena are of the most simple and the most regular nature ; and the torrid zone must, on these grounds, attract the principal notice of observers.

It is only between the tropics that it has been possible to determine the laws which regulate the small hourly variation of the barometer ; it is in the torrid zone that dry and wet seasons, and that the direction of the winds peculiar to each season, are submitted to invariable laws.

M. Humboldt has paid much attention to the relation between the declination of the sun and the commencement of the rainy season in the north part of the torrid zone. In proportion as the sun approaches the parallel of any place, the northern breezes are changed for calms, or south easterly winds. The transparency of the air is diminished, the unequal refrangibility of its strata causes those stars to twinkle which are 20° above the horizon. The vapours soon collect in clouds ; positive electricity is no longer constantly to be found in the lower part of the atmosphere, thunder is heard during the day, heavy rain succeeds, the calm of night is only interrupted by gales from the south-east.

M. Humboldt explains these appearances by the greater or less inequality between this part of the torrid zone and the neighbouring part of the temperate zone. When the sun is to the

south of the equator, it is winter in the northern hemisphere. The air of the temperate zone is then as different as it can be from that of the torrid zone. There flows into the latter a constant, cool, and uniform breeze, which carries the heated and moist air into the higher regions, from whence it flows back towards the same temperate zone, reestablishes the equilibrium, and deposits its moisture there ; so that the mean heat is always 5° or 6° less in the dry season than in the rainy ; but the southeast winds do not act like those of the north ; because they come from a hemisphere which contains much more water, and in which the upper current of air is not dispersed in the same manner as in the northern hemisphere.

M. Moreau de Jonnes has communicated some details, extracted from his correspondence, relative to the hurricane that caused so much damage in the Caribbee islands the 21st of last September. It was preceded by a dead calm, the wind shifted from north to north-west, and it was in this point that it blew with violence. M. de Jonnes remarks upon this subject, that in the preceding year the hurricane of the 20th of October came from the south west ; and that there exists a space of 90° between these two points, from south to north, from whence the wind never blows. The agitation of the air was followed by a great swell of the sea, which caused the shipping to drive ; yet no extraordinary movement was observed in the barometer. It is remarked, with some degree of sorrow, that the effect usually attributed to hurricanes, of purifying the air of the countries they devastate, was not verified upon this occasion, for the yellow fever did not cease to commit its usual ravages.

The same observer has also given some notices respecting the earthquakes which have been felt this year in the Caribbee Islands ; and which have had this peculiarity, that they have affected a kind of periodical recurrence. Eight of these earthquakes were felt from December to May ; one every month, except in April, in which month there were two ; and all of them took place in the night between nine o'clock and eleven.

MINERALOGY AND GEOLOGY.

M. Beudant continues to enrich crystallography with researches equally new and interesting. We saw last year, by his experiments, how a saline principle of a certain kind sometimes impressed its crystalline form upon a mixture in which it did not by any means form the greatest part.

He has occupied himself this year with a question that is not less important in respect to the knowledge of crystals ; namely, to determine the causes which occasion a saline substance, whose primitive molecules and nucleus have a constant form, to be disguised, by means of the accumulation of its molecules according to different laws, with so many and so various secondary forms that their number is sometimes astonishing.

Having remarked that the secondary forms of a substance are most commonly the same in any one mine or place where it is found associated with other minerals under similar circumstances; he judged that the secondary forms arise from the medium in which the crystallization takes place.

It has been known for a long time, from the experiments of Romé de Lille, and those of Fourcroy and M. Vauquelin, that the presence of urea occasions common salt to take an octahedral form, although in pure water it crystallizes in cubes, similar to its constituent molecules. The same substance produces an opposite effect upon muriate of ammonia, which crystallizes in octahedrons in pure water ; while urea causes it to crystallize in cubes.

A very slight excess or deficiency of base in alum causes it to assume either cubical or octahedral secondary forms ; and these forms are so truly secondary that an octahedral crystal of alum immerged in a solution which is richer in respect to its basis, becomes enveloped with crystalline layers, which cause it to assume at length the form of a cube.

Setting out from these facts, M. Beudant treats the question at full length, and has submitted the crystallization of salts to experiments made under every circumstance that he believed capable of exerting any influence upon it ; namely,

1. General and external circumstances ; such as heat, weight of the atmosphere, greater or less rapidity of evaporation, bulk of the solution, form of vessels, &c.

2. Mechanical mixtures which foul the solution, whether simply suspended in the state of an incoherent precipitate, or in that of a gelatinous deposit.

3. What he denominates chemical mixtures existing in one and the same solution.

4. Lastly, variations in the proportion of the constituent principles of the crystallized substance.

The circumstances of the first kind do not exercise any other action than what regards the size and perfection of the crystals. The case is even the same, as to any small portion of matter which remains in permanent suspension in the liquid ; but this cannot be said in respect to precipitates and chemical mixtures.

Crystals formed in the midst of an incoherent precipitate, deposited like mud at the bottom of a liquid, always take up a more or less considerable portion of the molecules of this precipitate, and by this admixture they usually lose all those small additional facets which would otherwise modify their predominant form. Thus the crystalline form acquires greater simplicity when it should apparently become more complicated ; at the same time the substances which would otherwise have yielded simple crystals still continue to yield them, and they do not receive any *modification.*

In a gelatinous deposit, crystals are rarely found in groups,

but almost always single, and of a remarkable sharpness and regularity of form, and they do not undergo any variations, but those which may result from the chemical action of the substance forming the deposit.

The variations that take place in crystals formed in a chemical mixture; that is to say, in a solution of another substance, are very numerous, even when this substance cannot be united with them. The above-mentioned phenomena are repeated in different forms: common salt crystallized in a solution of borax acquires truncations at the solid angles of its cubes; and alum crystallized in muriatic acid takes a form which M. Beudant has never been able to obtain in any other manner.

If the foreign substance dissolved in the liquid can be united in any proportion whatever with the crystal of another substance that is formed in it; and nevertheless the crystal, by its superior energy, determines the form of the constituent molecule, as we saw last year in the case of sulphate of iron, the matter in the solution will exercise in its turn some influence upon the secondary form of the crystal, and this influence usually consists in simplifying it, and causing the additional facets to disappear.

Thus 30 or 40 per cent. of sulphate of copper may be united to the rhomboidal crystallization of sulphate of iron, but it reduces this sulphate to a pure rhomboid without any truncation either of the angles or of the edges.

A small portion of acetate of copper reduces sulphate of iron to the same simple rhomboidal form, notwithstanding that this form is so disposed to become complicated with additional surfaces.

Other mixtures simplify in a less degree: thus the sulphate of alumine brings that of iron to a rhomboid with the lateral angles only truncated, or what M. Haüy calls his *variété unitaire;* and whenever this variety of green vitriol is found in the market, where it is very common, we may be sure, according to M. Beudant, that it contains alumine.

Lastly, the different proportions between the base and the acid, or, in double salts, between the two bases, produce very sensible effects upon the secondary forms, without altering the primitive form in the least. This has been already exemplified in respect to alum, and M. Beudant has verified it in many other salts.

The author of these researches has made some ingenious applications of these facts to the phenomena afforded by different crystalline mineral substances, upon which direct experiments cannot, in the present state of the science, be made; and he has exhibited some striking analogies: thus natural crystals mixed with foreign substances are in general more simple than others, as is shown in a specimen of axinite, or violet schorl, of Dauphiné, one extremity of which, being mixed with chlorite, is reduced to its primitive form; while the other end, which is pure, is varied by many facets produced by different decrements.

There is found rather abundantly in a ravine of the Mount d'Or, in Auvergne, fragments of a breccia, the hardness and other external characters of which having led to the supposition of its being of a siliceous nature, mineralogists did not pay much attention to it, except on account of some particles of sulphur which it sometimes contains in small cavities. .

M. Cordier, having submitted this breccia to different trials, found that it yielded by heat a notable proportion of sulphuric acid, and upon this important indication he proceeded to make a complete analysis of it, by which he found that this stone contained about 28 per cent. of silica, 27 of sulphuric acid, 31 of alumine, 6 of potash, and a little water and iron. These are very nearly the same ingredients as are found in the celebrated ore of Tolfa, which yields Roman alum. In reality, upon treating this breccia from the Mont d'Or, in the same manner as is practised at Tolfa ; that is to say, by breaking it, roasting, and exposing it to a moist air, from 10 to 20 per cent. of very pure alum was obtained from it; and this breccia even yielded alum without being roasted, but merely by exposure in a damp situation.

It is probable, from the researches made upon the spot by M. Ramond, that, with some pains, the beds from which the fragments scattered in the ravines were detached, may be discovered; and that quarries may be opened, the working of which cannot but be of advantage.

M. Cordier regards these sorts of stones as a mineralogical species, consisting essentially of sulphuric acid, alumine, and potash. The silica found in it is not essential for quarries of a stone not containing any silica, but all the other constituent principles exist at Montrone, in Tuscany, and yield the same products as that at Tolfa. Those varieties of this species in which silica enters, are easily distinguished by the jelly they form when they are treated in succession with caustic potash and hydrochloric acid diluted with water.

M. Cordier reduces to this species several volcanic stones, hitherto vaguely designated by geologists by the general denomination of altered lava.

Some country people in the department of the Lot, allured by the hope of finding pretended treasures, which are said to have been formerly buried by the English in certain caves in the neighbourhood of Breugue, have penetrated into these cavities, and having dug into and enlarged some crevices which they found at the bottom of them, they have discovered a deposition of bones, some of which belong to horses, others to that species of rhinoceros, of which so large a quantity of fossil bones have been found in Siberia, Germany, and England ; others again to a species of cervus not at present known to exist, and the horns of which have some slight analogy with those of a young rein-deer.

Guettard found a great number of these horns in the neighbourhood of Etampe.

These important witnesses of the revolutions of our continent have been collected by M. Delpont, Procureur du Roi at Figeac, and presented to the Academy by M. Cuvier. They are deposited in the King's cabinet.

M. Palissot de Beauvois has acquainted the Academy with a rather singular geological appearance, which he observed in the county of Rowan, in North Carolina. There is found in the middle of a hill formed of very fine sand mixed with small quartzose stones, and with numerous pieces of silver-coloured mica, a vein of stones so regularly placed that the inhabitants who, for a long time, have noticed the appearance, give it the name of the natural wall; and some naturalists have even maintained that it was a true wall, which might have been constructed in very remote ages by some people now unknown. The stones have generally four faces, are narrower at one of their ends, and have a small notch below their top. They are ranged horizontally. The kind of wall which they form is about 18 inches thick, its height in the place where it is uncovered is from six to nine feet, but upon digging into the ground, it has been followed to 12 and 18 feet deep, and it is already known to extend 300 feet, and even more, in length. A kind of argillaceous cement fills the intervals between the stones, and coats them externally; each of the stones is also covered with a layer of ochreous sandy earth.

M. de Beauvois has brought some of these stones to France, and upon being examined by the mineralogists of the Academy, they appeared to possess the characters of basalts; but as there has not as yet been found any traces of basalts or of volcanoes in the United States, and as the place where this wall is found is, generally speaking, of a primitive nature, it is possible that this pretended wall is nothing but a bed of trap; an amphibolic rock very similar to certain kinds of basalts.

We spoke in 1816 of the labours undertaken by M. Moreau de Jonnes to determine the geology of the Caribbee Islands, of the general ideas he had adopted on this subject, and of the particular description relative to Martinique and Guadaloupe, that he had presented to the Academy. He has continued the arrangement of his labours, and has read a paper upon the Vauclan, one of the most remarkable mountains in Martinique, not that it is the highest, but because it serves as a guide, and announces this island to navigators. It has not the form of a cone, hollowed at its top, but that of a prism resting on its side, or of an immense basaltic ridge; and M. de Jonnes considers it as part of the circuit and of the edge of a very large crater, the whole of which he thinks he has traced. The bottom of this crater is at present a valley not only fertile, but well cultivated.

The same author has given a geological description of Guadaloupe. He finds that the western island, in which there exists a solfatara in an active state, and the surface of which is about 67 square *leagues*, owes its origin to eruptions from four large submarine volcanoes, and that the eastern island, commonly called

Grande Ferre, is formed of a volcanic basis, covered with a thick bed of shell limestone. At Martinique, the eastern quarters are also covered with beds of marine limestone, either shelly or coralline.

The second part of La Richesse Minéralle of M. Héron de Villefosse, which was presented in manuscript to the Academy in 1816, has appeared this year in print, with an atlas. This work has justified the judgment that the commission passed upon it, and is become the indispensable guide of all those who are employed in the administration of mines, and in the works belonging to them.

(*To be continued.*)

Article XIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Succinic Acid.*

Dr. John, of Berlin, announces, that he has obtained succinic acid by the following process :

Two pounds of bread, a pound and a half of honey, as much of the fruit of the ceratonia siliqua, two pints of vinegar, as much spirits, and 28 pints of water, were treated in such a way as to obtain a liquid proper for undergoing the acetous fermentation. The vinegar produced was saturated with lime, and the acetate evaporated to dryness. Twenty-four ounces of this salt were triturated with an ounce of peroxide of manganese ; the mixture was put into a retort, and subjected to distillation, after having been mixed with 16 ounces of sulphuric acid diluted with 13 ounces of water. When no more acid came over, the receiver was changed, and the fire augmented. A sublimate then condensed in the neck of the retort which possessed the characters of succinic acid. When rectified, it crystallized in white flexible needles, and weighed two drachms.

John repeated this process two or three times, and always obtained succinic acid. The fruit of the ceratonia siliqua did not yield any succinic acid when subjected to analysis. Hence he is of opinion, that the succinic acid obtained was formed during the process.

II. *Animal Charcoal.*

Dobereiner has analyzed animal charcoal by heating it with peroxide of copper, and informs us that its constituents are :

$$
\begin{array}{ll}
\text{Carbon} \dots\dots\dots\dots\dots\dots\dots & 34\cdot2 \\
\text{Azote} \dots\dots\dots\dots\dots\dots\dots\dots & 13\cdot5 \\
\hline
& 47\cdot7
\end{array}
$$

This approaches very nearly to six atoms carbon and one atom azote; for

$$6 \text{ atoms carbon} \ldots \ldots \ldots = 4\cdot50$$
$$1 \text{ atom azote} \ldots \ldots \ldots \ldots = 1\cdot75$$

Now $4\cdot5 : 1\cdot75 :: 34\cdot2 : 13\cdot3$. If we suppose this analysis correct, the equivalent number for animal charcoal will be $6\cdot25$, or some multiple of it.

We are already acquainted with two other compounds of carbon and azote, pointed out and analyzed by Gay-Lussac.

1. Azoturet of carbon composed of three atoms carbon and one atom azote.

2. Cyanogen composed of two atoms carbon and one atom azote.

III. *Antidote for Vegetable Poisons.*

M. Drapiez has ascertained by numerous experiments that the fruit of the Fewillea cordifolia is a powerful antidote against vegetable poisons. This opinion has been long maintained by naturalists, but I am not aware that it was ever before verified by experiments made on purpose in any part of Europe. M. Drapiez poisoned dogs with the rhus toxicodendron, hemlock, and nux vomica. All those that were left to the effects of the poison, died; but those to whom the fruit of the fewillea cordifolia was administered, recovered completely after a short illness. To see whether this antidote would act in the same way, when applied externally to wounds into which vegetable poisons had been introduced, he took two arrows which had been dipped in the juice of manchenille, and slightly wounded with them two young cats. To the one of these, he applied a poultice, composed of the fruit of the fewillea cordifolia, while the other was left without any application. The former suffered no other inconvenience, except from the wound, which speedily healed; while the other, in a short time, fell into convulsions and died.

It would appear from these experiments, that the opinion entertained of the virtues of this fruit in the countries where it is produced is well founded. It would deserve in consequence to be introduced into our pharmacopœias as an important medicine; but it is necessary to know that it loses its virtues, if kept longer than two years after it has been gathered.

IV. *Nitrate of Silver.*

M. Brandenbourg has pointed out an economical method of separating silver from copper, or of making pure nitrate of silver, from an alloy of silver and copper, which is an object of some importance to practical chemists. His method is as follows:

He dissolves the alloy of silver and copper in nitric acid, and evaporates the liquid to dryness in a glass vessel. The salt is then put into an iron spoon, and exposed to a moderate heat, keeping the salt in a state of fusion till all ebullition is at an end.

It is then poured upon an oiled slab. To determine whether all the nitrate of copper has been converted into black oxide, a little of the salt is dissolved in water, and the solution is tested by ammonia. If the liquid, which ought to have been at first transparent and colourless, does not acquire the least tint of blue, we may conclude that it contains no copper. If there be still found traces of copper in the salt, we continue the fusion for some seconds longer. The salt by this treatment becomes black from the mixture of peroxide of copper with the nitrate of silver. To separate them, we have only to digest the salt in pure water. The nitrate of silver is dissolved, and constitutes a transparent and colourless solution, while the peroxide of copper remains behind. We have then only to evaporate the solution to obtain the nitrate of silver in the state of crystals.

V. *Gluten of Wheat.*

M. Taddey, an Italian chemist, has lately ascertained that the gluten of wheat may be decomposed into two principles, which he has distinguished by the names, *gliadine* (from γλια, gluten), and *zimome* (from ζυμη, ferment). They are obtained in a separate state by kneading the fresh gluten in successive portions of alcohol, as long as that liquid continues to become milky, when diluted with water. The alcohol solutions being set aside gradually deposit a whitish matter consisting of small filaments of gluten, and become perfectly transparent. Being now left to slow evaporation, the gliadine remains behind, of the consistence of honey, and mixed with a little yellow resinous matter, from which it may be freed by digestion in sulphuric ether, in which gliadine is not sensibly soluble. The portion of the gluten not dissolved by the alcohol is the *zimome.*

VI. *Properties of Gliadine.*

When dry, it has a straw-yellow colour, slightly transparent, and in thin plates, brittle, having a slight smell, similar to that of honeycomb, and, when slightly heated, giving out an odour similar to that of boiled apples. In the mouth, it becomes adhesive, and has a sweetish and balsamic taste. It is pretty soluble in boiling alcohol, which loses its transparence in proportion as it cools, and then retains only a small quantity in solution. It forms a kind of varnish in those bodies to which it is applied. It softens, but does not dissolve in cold distilled water. At a boiling heat it is converted into froth, and the liquid remains slightly milky. It is specifically heavier than water.

The alcoholic solution of gliadine becomes milky, when mixed with water, and is precipitated in white flocks by the alkaline carbonates. It is scarcely affected by the mineral and vegetable acids. Dry gliadine dissolves in caustic alkalies and in acids. *It swells* upon red-hot coals, and then contracts in the manner *of animal* substances. It burns with a pretty lively flame, and

leaves behind it a light, spongy charcoal, difficult to incinerate. Gliadine, in some respects, approaches the properties of resins; but differs from them in being insoluble in sulphuric ether. It is very sensibly affected by the infusion of nutgalls. It is capable of itself of undergoing a slow fermentation, and produces fermentation in saccharine substances.

VII. *Properties of Zimome.*

The gluten, thus treated by alcohol, is reduced to the third part of its former bulk. This diminution is owing not merely to the loss of the gliadine, but likewise to that of water. The residue is zimome, which may be obtained pure by boiling it repeatedly in alcohol, or by digesting it in repeated portions of that liquid cold, till it no longer gives out any gliadine.

Zimome thus purified has the form of small globules, or constitutes a shapeless mass, which is hard, tough, destitute of cohesion, and of an ash-white colour. When washed in water, it recovers part of its viscosity, and becomes quickly brown when left in contact of the air. It is specifically heavier than water. Its mode of fermenting is no longer that of gluten; for when it putrefies, it exhales a fetid urinous odour. It dissolves completely in vinegar and in the mineral acids at a boiling temperature. With caustic potash it combines and forms a kind of soap. When put into lime-water, or into the solutions of the alkaline carbonates, it becomes harder, and assumes a new appearance without dissolving. When thrown upon red-hot coals, it exhales an odour similar to that of burning hair or hoofs, and burns with flame.

Zimome is to be found in various parts of vegetables. It produces various kinds of fermentation, according to the nature of the substance with which it comes in contact.

VIII. *Prussic Acid in Consumptions.*

Most of our readers are aware that M. Magendie some time ago proposed prussic acid as a remedy in incipient consumption. In a disease of so desperate a nature, and which has hitherto baffled all the efforts of medical men, every new proposal is entitled to attention. This induces me to lay M. Magendie's formula before my medical readers.

He mixes one part of pure prussic acid (or hydrocyanic acid) prepared according to Gay-Lussac's method, with 8·5 parts of water by weight. To this mixture he gives the name of medicinal prussic acid. The state in which this substance is given to patients will be seen by the following formula:

℞ Medicinal prussic acid .. 1   gros, or   59·00 grs. troy
     Distilled water .,...... 1   lb.   or 7560·00 grs.
     Pure sugar . ... 1¼ oz.   or   708·75 grs.

Mix these ingredients, and let the patient take a table spoonful every morning and evening.

IX. *Camphor.*

The peculiar characters of camphor are well known to chemists. Its great volatility, its strong smell, its fusibility when heated, its solubility in nitric acid, and in alcohol. In some respects it resembles the volatile oils ; but the nondecomposition of it by nitric acid, except when we employ a great deal of acid, and assist the action by heat, sufficiently distinguishes it from these bodies. It melts, when heated to 288°, and boils at the temperature of 400°.

I had the curiosity to analyze it by passing it slowly through red-hot peroxide of copper. By this process it was converted into carbonic acid and water. The first of these I collected over mercury and measured, while the second was intercepted by means of muriate of lime, and the quantity of it was made known by the increased weight of the salt. One grain of camphor, when thus treated, yielded 5·837 cubic inches of carbonic acid gas under the mean temperature and pressure, and 1·3 gr. of water. Hence the constituents of camphor are :

$$
\begin{array}{lr}
\text{Carbon in carbonic acid} \dots\dots\dots\dots & 0\text{·}738 \\
\text{Hydrogen in water} \dots\dots\dots\dots\dots & 0\text{·}144 \\
\text{Oxygen (to make up the deficiency)} \dots & 0\text{·}118 \\
\hline
& 1\text{·}000
\end{array}
$$

This approaches very nearly to

$$
\begin{array}{lrr}
8\tfrac{1}{2} \text{ atoms carbon} \dots & = 6\text{·}375 & \dots 73\text{·}91 \\
10 \text{ atoms hydrogen} \dots & = 1\text{·}250 & \dots 14\text{·}49 \\
1 \text{ atom oxygen} \dots & = 1\text{·}000 & \dots 11\text{·}60 \\
\hline
& 8\text{·}625 & 100\text{·}00
\end{array}
$$

X. *Prussiate of Iron.*

The nature of prussiate of iron not being hitherto determined in a satisfactory manner, I made some time ago the following experiments on it, which appear to me to explain its composition pretty completely. A quantity of pernitrate of iron was precipitated by prussiate of potash, and the deep blue precipitate was collected on a filter, well washed, and dried in a temperature not above 150°. It is well known that this salt catches fire and burns with the emission of a great quantity of ammonia, when exposed to a heat not greatly exceeding 212°. It cannot, therefore, be completely freed from water by heat ; but as there is no great difficulty in determining the weight of the peroxide of iron, and of the ferrochyazic acid which the powder contains, I consider the presence of a little moisture as of no great consequence.

1. To determine the quantity of peroxide of iron, I digested 20 gr. of the prussiate of iron in potash ley diluted with water over a sand-bath for 24 hours. The liquid was then drawn off, and the red sediment carefully washed and dried. It weighed 7·56 gr. and was pure peroxide of iron.

2. Being thus acquainted with the weight of peroxide of iron in 20 gr. of the powder, I calculated how much ferrochyazic acid was requisite to saturate this quantity of iron; and how much potash would be just sufficient to decompose 20 gr. of prussiate of iron. This quantity of potash, together with the 20 gr. of the prussiate of iron and a sufficient quantity of water, were put into a phial, and digested on the sand-bath for 24 hours. The whole was then thrown on a filter, and the peroxide of iron remaining on the filter was washed quite clean with distilled water. The liquid which passed through the filter had a yellow colour, and the taste and properties of a solution of prussiate of potash. Being evaporated to dryness, and exposed to a heat of about 212°, there remained 19·3 grs. of pure prussiate of potash. Now 19·3 grs. of prussiate of potash dried at the temperature of 212° contain 10·2 gr. of ferrochyazic acid. This of consequence is the quantity of ferrochyazic acid contained in the 20 gr. of prussiate of iron which I examined.

From the preceding experiments, it follows that prussiate of iron is composed of

Ferrochyazic acid......	10·20	...... 51·0
Peroxide of iron.	7·56	...... 37·8
Water.	2·24	...... 11·2
	20·00	100·0

Now if we consider (with Mr. Porrett) the weight of an atom of ferrochyazic acid as 6·75, and that of an integrant particle of peroxide of iron as 5, in that case prussiate of iron will be a compound of one atom ferrochyazic acid and one atom of peroxide of iron. If we were to suppose the weight of an atom of peroxide of iron to be 10, then in that case the salt would be a compound of two atoms acid + one atom peroxide. But I am disposed to embrace the first alternative in consequence of the following fact which is easily verified.

Dissolve protosulphate of iron in water, and mix the solution with some sulphuretted hydrogen gas. Then drop into it prussiate of potash. A white powder is thrown down, which is a neutral protoferrochyazate of iron, or a compound of an atom of ferrochyazic acid and an atom of protoxide of iron. Expose this salt while moist to the air, and it is gradually converted into perferrochyazate of iron (or prussian blue) simply by the absorption of oxygen.

It is well known to the manufacturers of prussian blue that the pigment is at first of a dirty pale blue, and that it acquires its intense blue colour by long and laborious washing in water. The reason of this is, that a considerable proportion of the salt made at first by them is in the state of protochyazate of iron, and it slowly becomes perchyazate by absorbing oxygen from the atmosphere. If we employ the pernitrate of iron instead of the sulphate, we form prussian blue, at once possessed of the requi-

site beauty and intensity of colour.   Manufacturers would probably shorten their process considerably by dissolving the sulphate of iron in water some months before they use it, and by keeping the solutions in shallow vessels exposed to the action of the atmosphere.

XI. *Hydrocyanate of Ammonia.*

When prussiate of iron (prussian blue) is exposed to a red heat in a copper tube, and the products received in glass jars standing over mercury, the glass jar becomes coated with transparent crystals, having the smell of hydrocyanic acid, and readily soluble in water.   When a drop of sulphuric acid is let fall into a concentrated solution of these crystals, an effervescence takes place, and a strong smell of hydrocyanic acid exhales.   When some soda is mixed with the aqueous solution of these crystals and heat applied, a strong smell of ammonia is perceived. Hence I consider the crystals as hydrocyanate of ammonia.   The effect produced by the solution of these crystals upon different metalline solutions was as follows.   It precipitated

1. Permuriate of iron,	Yellow.
2. Sulphate of copper,	White, with a light shade of blue.
3. Nitrate of lead,	White, precipitate redissolved by nitric acid.
4. Nitrate of mercury,	White, ditto.
5. Corrosive sublimate,	White, redissolved by agitation.
6. Sulphate of zinc,	White, slight.
7. Muriate of manganese,	Ditto, ditto.
8. Nitrate of silver,	White, redissolved by agitation.
9. Sulphate of nickel,	Greenish, slight.
10. Sulphate of cobalt,	Reddish, ditto.

These precipitates do not correspond with those indicated by Scheele; but he made use of hydrocyanic acid; not hydrocyanate of ammonia, which is probably the cause of the difference.

XII. *Kilkenny Coal.*

In the paper which I published last summer on the different species of pit-coal, I was obliged to leave out Kilkenny coal for want of the requisite specimens.   I have been lately enabled, by the kindness of a friend, to make up that deficiency.   I shall state here the result of my trials to determine the composition of this species of coal.

Its specific gravity was 1·4354.

One hundred grains of it, when burned completely in a muffle, left four grains of a reddish-brown light earth, not in the least acted on by acids.

One hundred grains, when heated for several hours in a *covered* platinum crucible left 86·7 grs. of coal not in the least *altered in its* appearance.   Hence the loss of weight was probably owing not to the dissipation of any volatile matter from the

coal, but to the combustion of part of it from the long continued action of the heat, as the crucible, though covered with a lid, was not impervious to air.

One grain of this coal, when mixed with peroxide of copper, and exposed to a strong red heat, formed 7·06 cubic inches of carbonic acid gas; and no water whatever nor azotic gas was evolved. Now seven cubic inches of carbonic acid gas contain 0·893 gr. of carbon. Hence the grain of coal consisted of

Carbon .... 0·893
Ashes. .... 0·040
Deficiency. ..... 0·067 which must have been oxygen.

Now 0·893 gr. carbon and 0·067 gr. oxygen, when converted into volumes, are very nearly

7   cubic inches carbon,
0·2 cubic inch oxygen.

Which is equivalent to

35 atoms carbon. .... = 26·25
2 atoms oxygen. ...... = 2·00
                              ——————
                              28·25

So that Kilkenny coal differs essentially in its composition from every other species hitherto examined.

XIII. *Geographical Positions on the Coast of Dalmatia determined by Capt. G. H. Smyth.*

		Latitude.			Long. E. Ferro.		
*	Corfu Fort Alexandre sur Vido..	39°	38′	5″	37°	35′	23″
+	Cape Bianco (Corfu)	39	20	50	37	46	35
+	Cape Drasti (Corfu)	39	47	10	37	21	45
+	Merleru (isle)	39	52	50	37	14	55
+	Fano (west point)	39	50	20	36	59	35
+	Port Palermo (the castle)	40	2	55	37	27	55
—	Strada Bianca................	40	8	45	37	17	15
—	Valde Orsa...................	40	19	12	37	0	20
*	Saseno (isle, on the hill)........	40	29	10	36	53	57
+	Aulona (Douanne)	40	27	15	37	6	5
—	Pointe Samana (centre)........	40	48	55	36	57	22
—	Cape Lachi (tower)	41	10	10	37	5	5
+	Durazzo (the highest marabut) ...	41	17	32	37	6	20
—	Cape Pali....................	41	23	5	37	3	59
+	Cape Rodoni.................	41	37	35	37	7	55
—	St. Jouan di Medua.	41	48	20	37	8	45
—	Duleigno (Marabut)...........	41	53	50	36	50	25
—	Antivari (Point)	42	2	17	36	46	10
*	Budua (St. Nicolas)	42	15	45	36	30	32
—	Point d'Ostro de Cattaro	42	23	30	36	10	35
+	S. Marco (isle, the fort) Ragusa.	42	37	45	35	44	20

(Correspondence Astronomique, i. 456.)

ARTICLE XIV.

Astronomical, Magnetical, and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·27″ North. Longitude West in time 1′ 20·93″.

Astronomical Observation.

March 29, Lunar eclipse. $\begin{cases} \text{Moon rose eclipsed.} \\ \text{End of eclipse} \ldots\ldots & 8^h \ 0' \ 9'' \ \text{Mean Time at Bushey.} \end{cases}$

Magnetical Observations, 1820. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
March 1	8ʰ 40′	24° 30′ 57″	1ʰ 30′	24° 35′ 04″		
2	8 30	24 32 07	— —	— — —		
3	8 35	24 30 26	1 25	24 37 39		
4	8 35	24 30 26	1 15	24 37 25		
5	8 40	24 30 50	1 30	24 39 49		
6	8 35	24 31 40	1 25	24 38 55		
7	8 40	24 33 08	1 30	24 41 12		
8	6 40	24 33 07	1 25	24 40 32		
9	8 40	24 32 05	1 30	24 43 09		
10	8 40	24 31 47	— —	— — —		
11	8 35	24 33 25	1 25	24 41 20		
12	8 40	24 32 26	1 35	24 39 53		
13	8 35	24 30 55	1 25	24 40 24		
14	8 35	24 31 48	1 25	24 39 22		
15	— —	— — —	— —	— — —		
16	8 40	24 32 00	1 20	24 39 19		
17	8 35	24 29 30	1 20	24 40 01		
18	8 40	24 29 56	1 25	24 39 44		
19	8 40	24 31 12	1 25	24 38 11		
20	8 35	24 31 34	1 25	24 38 25	6ʰ 05′	24° 33′ 28″
21	8 35	24 30 46	1 25	24 38 20	6 15	24 33 42
22	8 40	24 29 12	1 25	24 39 03	6 10	24 32 16
23	8 35	24 29 05	1 20	24 39 25	6 15	24 34 12
24	8 35	24 28 58	1 15	24 39 32	6 15	24 32 53
25	8 40	24 30 43	1 25	24 40 54	— —	— — —
26	8 40	24 28 46	1 45	24 39 32	6 10	24 32 57
27	8· 35	24 30 11	1 25	24 39 / 54	6 15	24 33 34
28	8 35	24 29 34	1 15	24 40 48	6 15	24 36 34
29	8 40	24 28 25	1 15	24 39 57	6 15	24 35 13
30	8 35	24 28 26	1 20	24 39 47	6 15	24 33 33
31	8 35	24 30 24	1 35	24 39 39	6 15	24 32 55
Mean for Month.	8 37	24 30 47	1 25	24 39 33	6 13	24 33 45

March 1st, the needles were unsteady; during the night of the *2d,* there was a violent storm of wind from N.W. accompanied *with* snow.

8

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
March		Inches.				Feet.		
	Morn....	29·048	38°	71°	NW by W		Cloudy	33½
1	Noon....	29·095	41	59	NW		Cloudy	42
	Even	—	—	—	—		—	—
	Morn....	28·460	—	76	NW		Stormy	—
2	Noon....	—	—	—	—		—	
	Even	—	—	—	—		—	—
	Morn....	29·285	29	72	N by W		Cloudy	
3	Noon....	29·326	32	65	NNE		Fine	34
	Even	—	—	—	—		—	24½
	Morn....	29·532	28	71	NNW		Very fine	
4	Noon....	29·510	34	64	NNE		Cloudy	35
	Even	—	—	—	—		—	25½
	Morn....	29·712	29	66	NE		Clear	
5	Noon....	29·727	33	58	ENE		Fine	35
	Even	—	—	—	—		—	25
	Morn....	29·694	28	75	NNE		Snow	
6	Noon....	29·668	33	63	NE		Sn. show.	34½
	Even	—	—	—	—		—	23
	Morn....	29·690	27	71	NNW		Cloudy	
7	Noon....	29·652	31	72	W		Snow	36
	Even	—	—	—	—		—	31
	Morn....	29·777	32	76	NE		Very fine	
8	Noon....	29·785	39	63	NW		Very fine	39⅓
	Even	—	—	—	—		—	27
	Morn....	29·708	30	68	W by S		Clear	
9	Noon....	29·659	44	63	SSW		Fine	45½
	Even	—	—	—	—		—	29
	Morn....	29·349	33	80	S		Cloudy	
10	Noon....	—	—	—	—		—	43
	Even	—	—	—	—		—	30
	Morn....	29·134	44	70	ESE		Clear	
11	Noon....	29·129	48	56	SE by S		Very fine	50⅓
	Even	—	—	—	—		—	30½
	Morn....	28·975	33	75	ESE		Fine	
12	Noon....	28·946	45	64	ESE		Fine	45
	Even	—	—	—	—		—	34⅔
	Morn....	29·115	38	80	W by S		Fine	
13	Noon....	29·232	47	56	W		Fine	48
	Even	—	—	—	—		—	35
	Morn....	29·611	40	89	SSW		Fog	
14	Noon....	29·662	52	74	WSW		Cloudy	52½
	Even	—	—	—	—		—	46½
	Morn....	29·800	—	—	WNW		Foggy	
15	Noon....	—	—	—	—		—	60½
	Even	—	—	82	—		—	45
	Morn....	29·929	46	83	ESE		Fog	
16	Noon....	29·912	52	66	NE		Cloudy	52⅔
	Even	—	—	—	—		—	37½
	Morn....	29·805	42	83	NW		Foggy	
17	Noon....	29·800	47	78	N		Sm. rain	48⅔
	Even	—	—	—	—		—	33⅔
	Morn....	29·890	36	73	NNE		Very fine	
18	Noon....	29·874	45	54	NE		Very fine	47
	Even	—	—	—	—		—	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
March		Inches.				Feet.		
	Morn....	29·787	38°	79°	NE by N		Cloudy	34
19	Noon....	29·757	42	64	NE by N		Showery	43
	Even	—	—	—	—		—	} 34
	Morn....	29·783	35	78	NE		Cloudy	} 34
20	Noon....	29·783	42	68	NE		Cloudy	44
	Even	29·770	41	66	SE by E		Cloudy	} 30
	Morn....	29·688	35	68	W by N		Cloudy	} 30
21	Noon....	29·600	44	58	WNW		Cloudy	47
	Even	29·525	44	60	WNW		Cloudy	} 38⅔
	Morn....	29·322	42	66	NW		Very fine	} 38⅔
22	Noon....	29·330	50	53	WNW		Fine	50
	Even	29·290	46	57	SW by W		Cloudy	} 42
	Morn....	28·848	42	73	W		Cloudy	} 42
23	Noon....	28·734	42	69	W		Hail	46
	Even ...	28·600	43	68	W		Stormy	} 37¼
	Morn....	28·605	40	67	W		Fine	} 37¼
24	Noon....	28·514	46	53	W		Cloudy	49
	Even	28·428	41	64	NE		Cloudy	} 30¼
	Morn....	28·744	35	60	N		Very fine	} 30¼
25	Noon....	28·863	46	54	NNW		Very fine	47
	Even	—	—	—	W		Snow	} 27⅝
	Morn....	29·264	34	71	SSW		Cloudy	} 27⅝
26	Noon....	29·200	40	82	SW by S		Rain	45
	Even	29·175	45	93	WSW		Rain	} 45¼
	Morn....	29·307	47	92	SW		Rain	} 45¼
27	Noon....	29·309	50	77	SW		Showers	51
	Even	29·309	48	82	SW		Cloudy	} 43
	Morn....	29·513	46	79	W by S		Fine	} 43
28	Noon....	29·541	55	60	W by S		Fine	55½
	Even	29·552	48	72	SW		Cloudy	} 45¼
	Morn....	29·633	50	78	WSW		Cloudy	} 45¼
29	Noon....	29·651	56	64	WSW		Cloudy	58
	Even	29·600	51	70	SSW		Clear	} 44
	Morn....	29·488	47	75	NW by N		Fine	} 44
30	Noon....	29·508	59	56	NW by N		Fine	59⅓
	Even	29·508	52	56	NW		Very fine	} 40⅓
	Morn....	29·550	45	60	WSW		Clear	} 40⅓
31	Noon....	29·515	56	51	WSW		Clear	57
	Even	29·185	50	54*	W by N		Clear	

Rain, by the pluviameter between noon the 1st of March,
and noon the 1st of April 0·246 inch. The quantity that fell
on the roof of my observatory, during the same period, 0·263
inch. Evaporation, between noon the 1st of March, and noon
the 1st of April, 4·17 inches.

ARTICLE XV.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER. Max.	Min.	THERMOMETER. Max.	Min.	Evap.	Rain.	Hygr. at 9 a. m.
3d Mo.								
March 1	N W	29·62	28·98	45	31		16	69
2	N W	29·79	29·02	35	27		—	83
3	N W	30·08	29·79	37	26		—	73
4	N E	30·31	30·08	38	26			82
5	N E	30·31	30·28	38	22		—	66
6	N	30·28	30 26	37	21			74
7	N	30·35	30·20	39	31		—	77
8	N E	30·35	30·28	44	26			81
9	W	30·28	29·98	49	30			78
10	S	29·98	29·76	47	26	·		78
11	S E	29·76	29·59	55	28			97
12	S E	29·68	29·59	47	36			69
13	N W	30·16	29·68	53	32			75
14	S W	30·33	30·16	58	47	55		88
15	N W	30·44	30·33	53	43			84
16	S E	30·44	30·34	55	29			98
17	S W	30·42	30·32	52	32			99
18	N E	30·42	30·33	51	32			84
19	N E	30·35	30·33	46	32		—	70
20	N E	30·35	30·25	48	30			79
21	N W	30·25	29·89	51	39	57		74
22	N	29·89	29·38	55	44		13	67
23	N W	29·38	29·10	53	39		—	67
24	Var.	29·20	28·91	52	33		—	67
25	N	29·79	29·20	49	26			64
26	S W	29·83	29·79	52	43		5	62
27	S W	30·01	29·83	55	45		3	91
28	W	30·15	30·01	61	47	56		86
29	S W	30·15	30·04	62	53			78
30	N W	30·07	30·04	63	29			76
31	Var.	30·08	30·04	63	38	34		79
		30·44	28·91	63	21	2·02	0·37	99—62

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month.—1. Cloudy: a strong gale of wind during the night. 2. High wind continues: some snow about seven, a. m.: very stormy night: much damage was done on this and the preceding night to buildings and garden walls in this neighbourhood. 3. The wind still very high from NW, though a fine clear morning. 4. Fine morning: *Cirrus.* 5. Fine. 6. Some snow in the morning. 7. Snow in the night. 8—10. Fine. 11, 12. Hoar-frost. 13. Cloudy: fine. 14—17. Overcast. 17. Thick fog. 18—20. Cloudy. 21. Lunar halo and corona at night, followed by wind and rain. 22. Cloudy and fine. 23. Windy. 24, 25. Cloudy. 26. Hoar-frost: showery. 27. Cloudy; light showers. 29. Fine: the lunar eclipse very well seen: a *Stratus* at night afterwards. 30. Fine: *Cirrocumulus: Cirrus.* 31. Hoar-frost: fine.

RESULTS.

Winds: NE, 6; N, 4; NW, 8; W, 2; SW, 5; S, 1; SE, 3; Vár. 2.

Barometer : Mean height

      For the month. .... 29·971 inches.
      For the lunar period, ending the 6th. .... 30·022
      For 14 days, ending the 1st (moon north) .... 29·911
      For 13 days, ending the 14th (moon south) .... 30·012
      For 14 days, ending the 28th (moon north)........ 29·961

Thermometer: Mean height

      For the month.................................... 41·387°
      For the lunar period, ending as above .... 35·362
      For 30 days, the sun in Pisces. .... 37·166

Hygrometer : Mean for the month 78
Evaporation.. 2·02 inch.
Rain .... 0·37

*** The mean temperature of this month at *Tottenham* (two days being supplied from the above observations) was 41·42°. The rain there was 0·53 : the mean of the *hygrometer*, 64°. The general atmosphere of the district, I suspect, was, in point of humidity, between the two: the instruments and their exposures differ too much to be fairly comparable.

Laboratory, Stratford, Fourth Month, 21, 1820.   L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1820.

ARTICLE I.

On the Bleaching Powder, usually called Oxymuriate of Lime.
By Thomas Thomson, M.D, F.R.S.

IN a preceding number of the *Annals of Philosophy*, vol. xiii.
p. 182, I stated very briefly the result of a set of experiments
which I had made on Mr. Tennant's bleaching powder. In these
experiments I employed nitrate of silver to determine the quan-
tity of chlorine contained in the soluble part of the salt ; but this
method is not capable of detecting the whole of the chlorine, at
least without precautions which I neglected. This was shown
in a satisfactory manner by M. Gay-Lussac in the observations
with which he favoured me on my mode of analysis (Ann. de
Chim. et de Phys. x. 425, and xi. 108). When nitrate of silver
is dropped into a solution of chloride of lime, the nitric acid
unites with the lime, while the chlorine and the silver are preci-
pitated in the state of chloride of silver. But the silver existed
in the nitrate in the state of an oxide, while in the chloride of
silver it is in the metallic state. What then has become of the
oxygen of the oxide of silver ? If after dropping nitrate of silver
into the solution of chloride of lime till no farther precipitation
take place, we pour off the supernatant liquid, and decompose it
by the application of a moderate heat, oxygen gas is disengaged,
and if the residual matter be dissolved in water acidulated with
nitric acid, we shall find that a portion of chloride of silver
remains behind. From this experiment, for which I am indebted
to Gay-Lussac, it is obvious that nitrate of silver does not preci-
pitate all the chlorine. We see likewise the reason of this, and

we see also what has become of the oxygen of the oxide of silver. One portion of the silver is reduced to the metallic state, and precipitates in combination with the chlorine; while another portion remaining in the state of oxide unites with chloric acid, into which a portion of the chlorine has been changed; and the chlorate of silver thus formed remains in solution in the liquid. It is easy to see how much of the chlorine is precipitated in this case, and how much of it is converted into chloric acid. Five-sixths of the silver must be reduced to the metallic state, and unite with chlorine; the remaining sixth will continue in the state of oxide, and will unite with one-sixth of the chlorine, which has been converted into chloric acid by uniting with five atoms of oxygen given out by the reduced silver. Thus it appears that when chloride of lime is analyzed by means of nitrate of silver, one-sixth of the chlorine is converted into chloric acid, and escapes detection.

I have, therefore, had recourse to the following method of analyzing chloride of lime, which I consider as accurate, and as attended with fully as little difficulty as the method which I employed formerly. My experiments were made upon a very large scale; but it is obvious that the scale might be very much diminished, without injuring the accuracy of the results.

2714 grains of bleaching powder recently prepared, but very moist, were put into a retort, the weight of which had been previously ascertained, and marked upon the glass with a diamond. To the beak of the retort was luted a bent tube to convey any gaseous products into glass jars standing upon the shelf of the water trough. The retort was put into the sandpot of a furnace, and was gradually heated nearly to redness, and kept at that temperature till all the gaseous products ceased to come over. During this process, which lasted about four hours, there were extricated the following gases:

Oxygen gas .... 333·48 cubic inches.
Azotic gas .... 20·33

corrected to the mean pressure and temperature.

Grains.

Now 333·48 cubic inches of oxygen gas weigh 113·00
    20·33 cubic inches of azotic gas weigh.. 6·02

Total weight .... 119·02

The loss of weight sustained by the bleaching powder amounted to 875 grs.; consequently the water driven off from the powder by the distillation amounted to 755·98 grs.

I presume that the reader is aware that it is already known that the bleaching powder is a combination of chlorine and lime, or a chloride of lime. I suppose him aware also that when lime is heated to redness, and a current of chlorine passed through it,

the lime is decomposed, giving out its oxygen; while the chlorine uniting to the calcium constitutes chloride of calcium. I suppose him aware likewise that an atom of oxygen is equivalent to half a volume, and an atom of chlorine to a whole volume. Hence in the above process, for every volume of chlorine that unites to the calcium, half a volume of oxygen gas is disengaged. We can, therefore, easily determine how much chlorine exists in chloride of calcium, provided we know the quantity of oxygen gas disengaged from the lime during the formation of chloride of calcium; for the bulk of the chlorine will be just double that of the oxygen gas.

Now when chloride of lime is exposed to heat, the lime is decomposed, in consequence of the superior affinity of the chlorine for calcium at a high temperature. The lime gives out its oxygen, and the chloride of lime is converted into chloride of calcium. This is what happened in the preceding experiment. It is obvious, therefore, that in order to determine the volume of chlorine contained in the 2714 grs. of bleaching powder, we have only to double the volume of oxygen gas disengaged. We have seen that the oxygen amounted to 333·48 cubic inches; therefore, the chlorine must have amounted to 666·96 cubic inches; but the specific gravity of chlorine gas is 2·500, and 100 cubic inches of it under the mean temperature and pressure weigh 76·25 grs. Of consequence, 666·96 cubic inches of chlorine gas are equivalent to 508·557 grs.

Having thus determined the quantity of water and of chlorine which the bleaching powder contained, it still remained necessary to ascertain whether any muriatic acid was present in it. For this purpose I digested the matter which remained in the retort in repeated portions of distilled water till all the muriate of lime (chloride of calcium) in it was dissolved; but as there was obviously an excess of lime in the bleaching powder, part of which would be dissolved by the water along with the muriate of lime, I caused a current of carbonic acid gas to pass through it till the lime was all separated in the state of carbonate. The liquid was then freed from this carbonate by the filter. The liquid which passed through the filter was a solution of muriate of lime in water. It weighed 34,343·75 grs. and its specific gravity was 1·026: 100 grs. of it, being evaporated to dryness, left 3·4 grs. of chloride of calcium dried in the temperature of 510°; therefore the quantity contained in the whole liquid amounted to 1167·6875 grs.

Now when this chloride of calcium is dissolved in water, it is converted into muriate of lime; and 1167·6875 grs. of chloride of calcium is equivalent to 1352·0592 grs. of muriate of lime. Now muriate of lime is a compound of one atom muriatic acid (4·625) + one atom lime (3·625); therefore, 1352·0592 grs. of muriate of lime contain 757·973 grs. of muriatic acid, and 594·0862 grs. of lime.

We have seen that the chlorine contained in the bleaching salt weighed 508·657 grs. Now if we suppose this chlorine to be converted into muriatic acid, it would increase in the proportion of 4·5 : 4·625 ; so that it would become 522·684 grs. Thus the muriatic acid which was extracted from the bleaching powder exceeds that which would have been formed by the conversion of the chlorine into muriatic acid by (757·973 − 522·684 =) 235·289 grs. This quantity must have existed in the bleaching powder in the state of muriatic acid, and it must have been in combination with lime. Now 235·289 grs. of muriatic acid require for saturation 184·4 grs. of lime ; consequently the bleaching powder contained 419·689 grs. of muriate of lime, equivalent to 362·46 grs. of chloride of calcium.

It was first demonstrated by Mr. Dalton, and has since been amply confirmed by my own experiments, and by those of other chemists, that bleaching powder (supposing it pure) is a compound of one atom chlorine + two atoms lime. Now the chlorine contained in the bleaching powder weighed 508·557 grs. and it was combined with (594·0862 − 184·4) × 2 = 819·3724 grs. of lime. This agrees very nearly with the theoretical number, which is 818·4538.

From the preceding analysis, it appears that the 2714 grs. of bleaching powder, subjected to analysis, contained the following ingredients :

Subbichloride of lime	1327·9294
Water.	755·9800
Muriate of lime.	419·6890
	2503·5984
Loss	210·4016
Total	2714·0000

This loss was found among the residue which remained on the filter when the liquid containing the muriate of lime was separated from the undissolved portion. This undissolved portion weighed 697·6 grs. It contained, however, half the lime that had been united to the chlorine, and which we have seen above amounted to 409·6862 grs.; so that there appears to be a surplus amounting to 77·5122 grs.: but this was owing to the lime having absorbed carbonic acid while drying in the open air. Accordingly 100 grains of this residue being dissolved in nitric acid lost 11·5 grs. in weight, so that the whole would have lost 80·224 grs. which comes within 2½ grs. of the surplus.

On analyzing this white powder on the filter, I found it to be a mixture of quicklime, carbonate of lime, and a little clayey matter, with which the lime no doubt had been contaminated. If we reduce the substances found by the preceding analysis to 100 parts, we shall obtain the composition of our bleaching powder as follows :

Subbichloride of lime .... 48·93
Muriate of lime. .... 15·46
Water. .... 27·86
Uncombined lime and impurity ...... 7·75
                                    ————
                                    100·00

The most surprising thing connected with this analysis is the very great proportion of water which the bleaching powder contained, amounting to 27·86 per cent. I do not believe that the whole of this quantity could have been present in it when it was originally prepared. The 45·7 grs. of lime which 100 grs. of the bleaching powder contained, supposing it in the state of slacked lime, or hydrate of lime, when originally employed, as it undoubtedly was, would have contained only 10 grs. of water, or little more than one-third of the quantity of water really present in the powder. I am disposed to ascribe this surplus of water to some accidental exposure of the bleaching powder to moisture, during a voyage from Belfast, where it was manufactured, to Glasgow, where I analyzed it.

The reader would observe, that besides the oxygen gas, I obtained, during the exposure of the bleaching powder to heat, $20\frac{1}{3}$ cubic inches of azotic gas, or six grains. That the reader may understand the way in which this azotic gas came to be mixed with the oxygen gas, and the correction which must be introduced into the analysis in consequence, it will be necessary to explain how the volume of oxygen gas and of azotic gas was determined. After the process was at an end, the volume of gas extricated was easily determined, as it had been received into glass jars graduated to cubic inches. I merely reduced the volume thus found to what it would have been had the thermometer stood at 60°, and the barometer at 30 inches. To determine the volume of common air which was mixed with the oxygen gas thus evolved, the method which I took was this: After the whole apparatus was cold, and the retort with its contents weighed, in order to know the loss of weight which the bleaching powder had sustained, I replaced the retort in the sandpot, and raised the fire to the same intensity as it had been at during the experiment, and I kept it in this state as long as any common air continued to be driven into a graduated glass jar standing over the water trough on purpose to receive it. The volume of air thus driven out of the retort amounted, estimated at the mean temperature and pressure, to $27\frac{1}{2}$ cubic inches. This quantity of consequence was deducted from the volume of gas obtained; because it is obvious that this volume of common air must have been driven out of the retort by the heat, and that it must have mixed itself with the oxygen gas in the jar. Now $27\frac{1}{2}$ cubic inches of common air are composed of

Oxygen. .... 5·775 cubic inches.
Azote. .... 21·725
                                ————
                                27·500

The next step was to determine the purity of the oxygen gas obtained from the bleaching powder. This was easily done by mixing a measured quantity of it with hydrogen gas, burning the mixture by means of an electric spark, and noting the diminution of bulk. One-third of this diminution denotes the volume of oxygen gas present in the mixture before the combustion. By this method, I ascertained the proportions of oxygen and azote that existed in my gas. I may state the results. The gas was received in three jars of unequal sizes. The gas in these three jars was composed as follows :

First jar ...... 83⅓ oxygen + 16⅔ azote.
Second jar .... 91·6   + 8·4
Third jar...... 93⅓   + 6⅔

Knowing the volume of gas in each jar, it was easy to determine the absolute quantity of azotic gas present. It amounted to 42·05 cubic inches. Deducting the 21·725 cubic inches which existed in the common air of the retort driven off by heat, there remained a surplus of 20·33 cubic inches of azote.

A very simple consideration of the phenomena of the process will enable us to account for this surplus of azotic gas. The extrication of the oxygen gas from the bleaching powder lasted four hours. It was accompanied by the extrication of 755·98 grains of water, which came off chiefly, if not entirely, during the first hour, and it was totally converted into steam. Now 755·98 grains of water are very nearly 2·98 cubic inches ; and when water is converted into steam, its bulk increases about 1700 times ; so that the bulk of the steam from this quantity of water would be 5066 cubic inches. All this quantity would pass successively through the retort : the consequence would be that a much greater proportion of the common air of the retort would pass into the receiver than would have been driven out by the simple expansion caused by the heat ; this air in the retort being replaced by the steam ; but after the steam ceased to form, it would be driven out of the retort in its turn by the oxygen gas that still continued to be evolved ; so that ultimately the retort would contain only pure oxygen gas expanded to the degree produced by the heat to which the retort was exposed. That this was really the case was obvious from this circumstance. After the retort was allowed to cool, it was found perfectly dry ; of consequence, all the steam had been driven out of it. The surplus of azote then found in the gas had been driven out of the retort by the steam, and its place in the retort had been ulti-

mately supplied by a quantity of oxygen gas exactly equal to it
in volume; consequently we ought to have increased the produce
of oxygen gas by 20·33 cubic inches of oxygen gas. This indi-
cates an additional quantity of chlorine in the bleaching powder
equivalent to 40·66 cubic inches, or 31 grs. Now 31 grs. of
chlorine, in order to be converted into subbichloride of lime,
must unite with 50 grs. of lime. We must, therefore, add 81
grains to the quantity of subbichloride of lime which we found
in the bleaching powder, and subtract 50 grains from the quan-
tity of uncombined lime, which we stated it to contain. This
will make the true constitution of the bleaching powder as
follows:

Subbichloride of lime	1408·9294
Muriate of lime.	419·6890
Water.	755·9800
Loss	129·4016
	2714·0000

The true composition then of the bleaching powder reduced
to 100 parts is as follows:

Subbichloride of lime	51·91
Muriate of lime.	15 46
Water.	27·86
Uncombined lime.	4·77
	100·00

So that rather more than half of the powder consisted of pure
subbichloride of lime, while the remainder consisted of matter of
no efficacy for bleaching.

I was long at a loss to account for the great proportion of
water which this specimen of bleaching powder contained; but
I now conceive that the following explanation will enable us to
account for it. The sulphuric acid used in the process was very
weak; namely, about the specific gravity 1·5, instead of 1·75,
which is the specific gravity required by law in Great Britain.
The consequence would be that a portion of the water of the
acid would be driven over into the receiver along with the chlo-
rine gas by the heat required to disengage that gas.

The extraordinary strength of the specimen, which will sur-
prise both manufacturers and bleachers, and which, I have reason
to believe, greatly exceeds what can possibly be made in the
large way by the usual process, is most probably owing to its
having been taken from the surface of the lime exposed on the
bottom of the receiver, which is always much nearer a state of
saturation than any other portion of the powder. The specimen
examined, therefore, serves rather to show to what strength the
bleaching powder can be made than to give a fair sample of

what is usually exposed to sale. But I have likewise subjected to analysis a quantity of bleaching powder, which I have reason to believe constitutes a pretty fair specimen of the average strength at which the powder is usually exposed to sale. The analysis was conducted precisely as the former, and upon nearly as great a scale. To be quite sure of extricating the whole oxygen gas from the powder, I raised it in the retort to an incipient red heat, and kept it in that state for a considerable time.

The quantity of bleaching powder analyzed was 1123 grains. I found its constituents as follows:

        Subbichloride of lime .... 410·135
        Muriate of lime. ......,....... 207·720
        Water. .... 190·100
        Uncombined lime, and impurity.... 315·045
                                         ─────────
                                          1123·000

When these are reduced to the proportional numbers for 100 parts of the powder, we find it constituted as follows:

        Subbichloride of lime .... 36·52
        Muriate of lime. ....,... 18·50
        Water. .... 16·93
        Uncombined lime, &c. .... 28·05
                                          ───────
                                          100·00

The great difference between this powder and the former consists in the quantity of uncombined lime. The proportion of muriate of lime is nearly the same in both. This I think renders it probable that the muriate of lime is formed, at least chiefly, during the process of manufacturing the bleaching powder. Both of these samples were quite fresh, and were transported into my laboratory in bottles well sealed up.

Besides the variation which frequently occurs in the original quality of the powder, arising sometimes from defective saturation of the lime with chlorine, and sometimes from a conversion of that gas into muriatic acid, or to these two causes combined, it is also subject to deterioration by keeping, being gradually converted into muriate of lime.

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ARTICLE II.

On the Composition of Chloride of Sulphur. By Thomas Thomson, M.D. F.R.S.

THIS substance was discovered by me in 1803, and described in a paper published in Nicholson's Journal, for October, 1803,

entitled " On the Compounds of Sulphur and Oxygen." In that paper, I attempted an analysis of it; but at that time the data upon which I had to go were so inaccurate that nothing more than an imperfect approximation could be expected. There was one fact, however, which I pointed out, which has been totally overlooked since, though it is of considerable importance, and though it will serve to account for variations that have been observed in the properties of chloride of sulphur by the few chemists who have published detailed accounts of its nature. The fact to which I allude is that it varies in its composition according to the proportion of the constituents employed in forming it. Thus, for example, no two chemists have stated the specific gravity of the chloride of sulphur the same.

I found the specific gravity of the first portion of it which I prepared to be................................ 1·623*
    Berthollet, jun. found it .... 1·7†
    Bucholz. .... 1·699‡
    Sp. gr. of a new portion lately made by me. . 1·6789

I conclude from the preceding table, that the composition of the portion of chloride of sulphur, which served for my experiments in the original paper which I published on the subject, was different from that whose specific gravity was 1·699.

It will be seen, by consulting my paper already referred to, that one part of sulphur, when converted into chloride of sulphur, weighed 2·63 parts. Now as a portion of the sulphur was volatilized during the process, and as the chloride was not rigidly weighed (for at that time I was not in possession of a pair of scales large enough to weigh the Woulfe's bottle containing the chloride, and I was, therefore, obliged to pour it into another smaller vessel); but a certain portion of it lost; the probability is, that my chloride was a compound of one atom of sulphur and one atom of chlorine. Had no sulphur been volatilized, and had the chloride been a compound of an atom of sulphur and an atom of chlorine, then one part of sulphur would have been converted into 3·25 of chloride of sulphur. The quantity which Berthollet obtained approaches very nearly to the requisite weight; for when his products are calculated by the correct data at present in our possession, it will be seen that one part of sulphur was converted by him into 3·123 parts of chloride of sulphur. § Bucholz, on the contrary, obtained from one part of sulphur only 2·11 parts of chloride of sulphur. This approaches nearest to what I obtained, and shows either that his process had not been carried so far as that of Berthollet, or that a greater loss had been sustained by him. ‖

Neither my analytical experiments, nor those of Berthollet or

* Nicholson's Journal, vi. 104.   † Memoires d' Arcueil, i. 166.
‡ Gehlen's Journal, second series, ix. 176.   § Memoires d' Arcueil, i. 165.
‖ *Gehlen's Journal, second series,* i. 175.

Gehlen, are of such a nature as to exhibit the real composition of this substance. Indeed a good analytical result was at that time unlikely; not merely because we were ignorant of the true nature of the compound, but because the curious suite of changes which it undergoes, when left in contact with water, could not be understood till the nature and properties of hyposulphurous acid had been investigated.

Before proceeding to the analysis of chloride of. sulphur, which is to constitute the subject of this paper, I beg leave to return my thanks to Mr. Herschell for his paper on the hyposulphurous acid, published in the first volume of the Edinburgh Philosophical Journal. It is an excellent paper, and does great credit to the author both as an experimenter and a philosopher. I was myself aware of a good many of the facts. contained in that paper before its publication, but by no means of the whole of them. Indeed it was a fact contained in it, new to me, that suggested to me the new analysis of the chloride of sulphur which I am going to describe. The fact to which I allude is the gradual decomposition of hyposulphate of silver, when left in solution in water, and its conversion into sulphuret of silver. This put me in mind of a phenomenon which I had observed in my former rude attempts to analyze the chloride of sulphur, and which I was unable to account for—I mean the change of colour which the precipitate (from water in which the chloride of sulphur has been agitated), by means of nitrate of silver, undergoes from white to brownish-black. The knowledge of the spontaneous change of hyposulphite of silver into sulphuret of silver suggested the cause of this change, and induced me to repeat my old analysis with the requisite attention to precision; so true is it that no addition can be made to any part of chemistry, which has not a tendency to throw light upon other parts of the science apparently totally unconnected with the new discoveries.

42·9 grs. of chloride of sulphur were dropped into about eight ounces of distilled water contained in a common phial. The phial was immediately corked tightly, and it was agitated violently at intervals for several weeks. The chloride at first fell to the bottom of the liquid. It was partially decomposed, and flocks of sulphur separated by the agitation; but the decomposition was very slow, and at least six weeks elapsed before all traces of the chloride disappeared. There was then a quantity of sulphur collected at the bottom of the phial, and the water had a considerable opalescence, owing apparently to a portion of sulphur suspended in it in the state of a very minute powder. I allowed it to remain at rest for a week to give the sulphur time to subside; but finding it continue as opalescent as ever, I poured into it a quantity of ammonia rather more than sufficient to saturate all the acid which it contained, and threw the whole upon a filter. The liquid passed through colourless, and the

quantity of sulphur collected on the filter weighed, when dry, 17·46 grs.

The liquid was neutralized by means of acetic acid, and some nitrate of barytes was dropped into it. No immediate effect was produced; but after the liquid had stood for 24 hours, a slight precipitate was deposited on the bottom of the glass containing the liquid. This precipitate was insoluble in nitric acid. After being washed and dried, it was found to weigh two grains. I considered it to be sulphite of barytes. On that supposition, it is equivalent to 0·29 gr. of sulphur. Thus the sulphur obtained from 42·9 grs. of chloride of sulphur amounted to 17·75 grs.

Into the liquid thus freed from sulphuric and sulphurous acids I dropped a solution of nitrate of silver as long as any precipitate continued to fall. The precipitate which first appeared had the colour and characters which usually distinguish chloride of silver; but when the liquid had stood for some time, it always deposited a brownish-black matter, similar in appearance to sulphuret of silver. Several days elapsed before the nitrate of silver had precipitated the whole of the precipitable matter, and the proportion of sulphuret of silver increased considerably towards the latter parts of the process. Indeed the sulphuret continued to fall after all the chloride of silver had been thrown down. In order to ensure getting the whole of the sulphuret, an excess of nitrate of silver was finally introduced into the liquid, and the mixture was allowed to remain till the liquid became perfectly clear, and continued so. The liquid was now drawn off with a syphon, and the precipitate of silver was washed with distilled water till it was perfectly clear.

This precipitate consisted obviously of two distinct substances; namely, chloride of silver, and sulphuret of silver. To separate them from each other, I digested the whole precipitate in a sufficient quantity of caustic ammonia to dissolve the whole of the chloride of silver which it contained. The ammoniacal solution was separated from the sulphuret of silver by throwing the whole upon the filter. The sulphuret of silver being sufficiently washed and well dried weighed 15·13 grs.

Now sulphuret of silver is a compound of 2 sulphur + 13·75 silver. It is obvious, therefore, that 15·13 grs. of sulphuret of silver contain 1·92 gr. of sulphur; therefore the whole sulphur obtained from the 42·9 grs. of chloride of sulphur amounts to the following quantity:

		Grains.
1. Deposited from the water		17·46
2. In two grains sulphite of barytes ..		0·29
3. In 15·13 grs. of sulphuret of silver.		1·92
		19·67

Now 19·67 grs. approaches to half the weight of 42·9 grs. which was the whole chloride of sulphur subjected to analysis.

The ammoniacal solution of chloride of silver, being saturated with muriatic acid, let fall the chloride of silver which it contained. This chloride, being well washed and dried, was found to weigh 83·7 grs.; but as chloride of silver is a compound of 4·5 chlorine + 13·75 silver, it is obvious that 83·7 grs. of it contain 20·63 grs. of chlorine. This of course is the quantity of chlorine contained in the 42·9 grs. of the chloride of sulphur subjected to analysis.

From the preceding details, it is obvious that the chloride of sulphur was composed of

Chlorine	20·63	48·09
Sulphur.	19·67	45·85
	40·30	
Loss	2·60	6·06
	42·90	100·00

The loss of six per cent. incurred during this analysis will not perhaps be thought excessive, if the length of time which the analysis took up, and the volatile nature of the chloride of sulphur, be considered. It is probable that the greatest part of this loss was owing to the escape of a portion of the muriatic acid into which the chlorine was converted by agitating the chloride of sulphur with water. Let us now see what inferences respecting the constitution of chloride of sulphur we are entitled to draw from the preceding analysis. An atom of sulphur weighs 2, and an atom of chlorine, 4·5. Hence it is obvious that our chloride of sulphur was not a compound of one atom sulphur + one atom chlorine; otherwise the chlorine, instead of merely exceeding the sulphur a little in quantity, would have been more than double its weight. If we suppose the chloride examined to have been a compound of two atoms sulphur + one atom chlorine, then the weights of the sulphur and chlorine would be to each other as 4 to 4·5; but 4 : 4·5 :: 45·85 : 51·58. Therefore, if the quantity of chlorine obtained in the preceding analysis had been 51·58 instead of 48·09, the chloride would have been composed exactly of two atoms sulphur + one atom chlorine. Now I have little doubt that from the greater volatility of the chlorine when compared to the sulphur, the greater part of the loss would be owing to the escape of it, or of the muriatic acid gas, into which it was converted. I conceive myself, therefore, entitled to conclude that the chloride of sulphur which I subjected to analysis was a compound of two atoms sulphur + one atom chlorine. It was, therefore, a subbichloride of sulphur.

It is scarcely necessary to say that I repeated the preceding analysis a second time without coming any nearer the truth. I conceive it, therefore, quite unnecessary to state the details of *the other* analysis. It was conducted precisely in the same way

as the preceding, and the phenomena which it exhibited were precisely the same.

Now that we are acquainted with the composition of the sub-bichloride of sulphur, which I subjected to analysis, the phenomena which presented themselves during the analysis, and which I have partly noticed above, admit of an easy and obvious explanation. When the subbichloride is agitated strongly in water, it gradually deposits one half of its sulphur. The remaining half of the sulphur is to the chlorine in the proportion of one atom to one atom. Water is decomposed by the mutual action of the chlorine and the sulphur which continues united to it. The oxygen of the water unites to the sulphur, and converts it into hyposulphurous acid, while the hydrogen of the water unites to the chlorine and converts it into muriatic acid.

Thus one half of the sulphur is deposited while the other half is converted into hyposulphurous acid. The hyposulphurous acid and the muriatic acid dissolve in the water. This decomposition does not take place at once, because, from the great specific gravity of the subbichloride of sulphur, and from the viscidity of the sulphur first evolved, great agitation and a considerable interval of time are necessary before the two liquids can come sufficiently in contact to act upon each other so as to produce mutual decomposition. New portions of hyposulphurous acid, therefore, and of muriatic acid, are formed at each successive agitation, and dissolved in the water.

But hyposulphurous acid, in order to be a permanent substance, requires to be united to a base. When merely dissolved in water, it speedily undergoes decomposition, letting fall sulphur, and probably being converted into sulphurous acid. Hence the reason why the liquid always continued opalescent: it was owing to the continual decomposition of the hyposulphurous acid, and the continual evolution of new portions of sulphur in proportion as the old portions subsided: but when the hyposulphurous acid was saturated with ammonia, its spontaneous decomposition was stopped. This was the reason why the liquid became transparent, and why I was able to separate all the sulphur from it when it was saturated with ammonia.

Hyposulphurous acid is not precipitated by the salts of barytes. Hence it was only the sulphurous acid which the liquid contained, and which had been formed at the expense of the hyposulphurous acid, that was precipitated by means of the nitrate of barytes.

Nitrate of silver does not precipitate hyposulphurous acid, the hyposulphite of silver being a soluble salt. Hence the reason why nitrate of silver only threw down the muriatic acid leaving the hyposulphite of ammonia still in the liquid, or rather converting it to hyposulphite of silver. But Mr. Herschell has shown that hyposulphite of silver is not a permanent salt, being gradually decomposed into water and sulphuret of silver. Hence

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the reason of the gradual deposition of sulphuret of silver, and hence the reason of the length of time necessary to throw down the acids in the d by means of nitrate of silver. The muriatic acid was immediately thrown down in the state of chloride of silver; but the hyposulphurous acid was converted at first into hyposulphite of silver, which slowly deposited sulphuret of silver. Thus the muriatic acid was thrown down the state of chloride of silver, while the sulphur of the hyposulphurous acid was precipitated in the state of sulphuret of silver.

Thus the action of water upon the subbichloride of sulphur cannot be completely understood till we know the properties of hyposulphurous acid and of some of its salts; but when these are once known, nothing is simpler or more beautiful than the series of decompositions which take place during the analytical experiments which I have described in this paper. It is not surprising then that I did not succeed in my attempts to analyze the chloride of sulphur in the year 1803. I at that time had no notion of the existence of hyposulphurous acid, and was not aware of any other acid compounds of sulphur, except sulphuric and sulphurous acids. It will be seen by consulting my paper published in 1803, that there was no sulphurous nor sulphuric acid in the chloride, but that one or other of them made their appearance when the chloride was agitated with water. It will be seen too that when the liquid was precipitated by nitrate of silver, the precipitate had a brown colour, and of course must have been contaminated with sulphuret of silver. I was not able at the time to form any notion of the source of this sulphuret, and did not even attempt to determine its quantity. Indeed I thought the quantity of it was so small as to be insignificant, as far as the analytical results were concerned; but the preceding details are sufficient to show us that this notion was very ill founded. Had I subjected 100 grs. of the subbichloride of sulphur to analysis, as I did in 1803, it is obvious that the weight of the sulphuret of silver formed during the analysis would have amounted to 35·27 grs.; while the chloride of silver would have amounted to 195·1 grs. Now if the 35·27 grs. of sulphuret had been reckoned chloride (as I did in the analytical experiments published in 1803), they would have indicated 8·7 grs. of chlorine; whereas in reality they indicate 4·47 grs. of sulphur. Thus the quantity of chlorine would have been overrated to the amount of 8·7 grs.; while the quantity of sulphur would have been made 4¼ grs. below the truth.

It was equally out of the power of Berthollet and Bucholz at the periods when they made their respective analyses to take a correct view of the phenomena. They were just as ignorant of the nature and properties of hyposulphurous acid as I myself had been. Of course their experiments do not lead us to any correct views respecting the constituents of the chloride of sulphur which they subjected to analysis; neither are they suscep-

tible of being corrected by the application of more recently discovered facts. Hence as far as the analysis of the chloride goes, their researches are quite useless. Had I taken the trouble to separate the chloride of silver from the sulphuret, and had I recorded the weight of each in my original paper, my experiments of 1803 might have been applied with ease to determine the true composition of the chloride of sulphur which I at that time subjected to analysis; but the neglect of these essential facts prevents the possibility of making any use of my experiments, and of course renders it quite unnecessary to correct them.

I am of opinion that whenever sulphur and chlorine are united together by my original process; namely, by passing a current of chlorine through flowers of sulphur till the whole is liquified, we always form a subbichloride of sulphur, or a compound of one atom chlorine with two atoms of sulphur. At least I have repeated the process three times, and each time the liquid formed was a subbichloride. I have not tried the effect of continuing the current of chlorine gas as long as it continues to be absorbed; but it would probably form a chloride of sulphur, or a compound of one atom chlorine and one atom sulphur. But in the chloride of sulphur which I prepared and attempted to analyze in 1803, the current of chlorine was continued a considerable time after the sulphur was liquified. Hence there was probably more chlorine in it than in the liquid, the analysis of which has been related in this paper. This was probably the reason why its specific gravity was different from that of the liquid obtained by Berthollet, and Bucholz, and from that which I employed for the present analysis. Berthollet, or at least Bucholz, had prepared subbichlorides of sulphur; while the liquid on which my original experiments were made was probably a simple chloride.

That a compound of one atom of chlorine and one atom of sulphur exists as well as a subbichloride of sulphur, appears to me sufficiently demonstrated from a synthetical experiment related by Sir H. Davy. He found that when dry sulphur is put into chlorine gas, the gas is absorbed by the sulphur, and a liquid chloride formed. Now he observed that 10 grs. of sulphur were just capable of absorbing 30 cubic inches of chlorine gas; but 30 cubic inches of chlorine gas weigh 22·875 grs. According to this experiment, 10 grs. of sulphur combine with 22·875 grs. of chlorine; of consequence, two grains of sulphur would combine with 4·575 grs. of chlorine. Now 2 is the weight of an atom of sulphur, and 4·575 only exceeds the weight of an atom of chlorine by 0·075 gr. An error of half a cubic inch in the quantity of chlorine absorbed by the sulphur would have produced this difference. I think, therefore, we may conclude, without any hesitation, that Davy's synthetical chloride was a compound of one atom chlorine + one atom sulphur.

Unfortunately Davy has not mentioned any of the properties of the chloride which he formed, though there is every reason to believe that its characters must differ in several particulars from those of the subbichloride of sulphur.

From the facts stated in this paper, we may conclude that chlorine and sulphur are capable of uniting at least in two proportions. These compounds are :

1. Chloride of sulphur composed of one atom chlorine + one atom sulphur, or of

Chlorine.	4·5	. . . . . .	69·23	. . . . . . 100·0
Sulphur	2·0	. . . . . .	30·77	. . . . . . 44·44
			100·00	

2. Subbichloride of sulphur composed of one atom chlorine + 2 atoms sulphur, or of

Chlorine.	4·5	. . . . . .	52·94	. . . . . . 100·00
Sulphur	4·0	. . . . . .	47·06	. . . . . . 88·89
			100·00	

It is not unlikely from the analogy of oxygen that sulphur may be capable of uniting likewise with two atoms and with three atoms of chlorine; but as these compounds are not likely to be of much utility, it might be considered as a waste of time to endeavour experimentally to obtain them.

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ARTICLE III.

Physico-chemical Inquiry into the Red Snow of the Environs of Mount St. Bernard. By M. Peschier.

THE singular phenomenon of the red snow has excited much attention, on account of that which the navigators in the first arctic expedition had observed, and even collected, in those high latitudes. Observations of the same kind have been called to mind which were formerly made by our De Saussure in his attempt to analyze the colouring matter which sometimes tinges the snow on the high mountains. A chemical and physical examination has lately been made in London of this substance brought from the arctic regions, and signs of organization even thought to be perceived in it. We have ourselves more than once observed this phenomenon on the snow on the Alps ; and a lover of mountains, who is nearly connected with us, and who, like us, has the advantage of living in correspondence with the

respectable prior of the Convent of Grand St. Bernard, has availed himself of this circumstance, and of the acquaintance of the learned monk with natural history, to address to him a series of questions on the subject. He has very obligingly answered them, and these answers seem to us to throw much light on the phenomenon as connected with the localities.

On the other hand, this same correspondent having been so kind as to send us, at two different times, samples of the colouring matter collected by himself with great care, we have requested M. Peschier, Member of the Physical and Natural History Society, and of the committee of chemistry of the Society for the Promotion of the Arts, to undertake the analysis of these samples. This he has been so kind as to do; and we think that our readers will be obliged to us for communicating to them these two papers, which those who feel interested in the question of the red snow will not find out of their place in our journal.

Questions and Answers respecting the Red Snow of the Environs of Mount St. Bernard.

Is the red snow permanent?

It is permanent.

Is it always seen in the same place?

It is always seen in the same place. It generally occupies the plateaux, commanded by declivities covered with snow.

Is it concealed by the snow which falls upon it, or does the latter become red by the contact?

It is concealed by the snow which falls on it, and the latter does not become red by the contact.

I have seen the red snow on the Buet, the St. Bernard, the Col de la Seigne, the Bonhomme: is it generally found at the same elevation as these summits in the other parts of the glaciers?

It is found at this level, and also higher and lower, provided there are masses of snow sufficiently large not to melt during the summer. It is sometimes found on the glaciers.

Is it more abundant at certain times?

After high south, or south-west winds.

Has it been noticed whether it was red at certain depths?

To the depth of two or three inches.

Would it not be possible to obtain by filtering it, a residue which might be analyzed?

Without having made the experiment, I believe that it would furnish an earthy and ferruginous residue which might be analyzed.

Is there any opinion on the cause of this colour?

I do not know that there are any opinions on this cause: it might be ascribed to the colour of the earth, especially the ferru-

ginous earth, which the winds carry away, and let fall on the snow, as elsewhere.

Does not the red snow give rise to certain superstitions?

Absolutely to none in our country; the people do not even pay attention to it.

Has it been observed whether it is more or less abundant according to the temperature of the air?

It has not been observed, but the more the season advances, the more abundant it is; because as the winds bring fresh supplies of earthy matter, they are the more easily perceived.

In particular, would it not be more or less abundant in proportion as the melting of the snow has been more considerable?

The melting of the snow and the rain occasion little streams, which flow over the snow and make furrows in it. In these furrows, hollows are formed, as there are in a rivulet; it is there that it is more particularly red, because the water, carrying with it the colouring matters, allows them to precipitate, on account of the diminution of the force of the current.

In what places is the red snow seen the most frequently, and in the greatest abundance?

It is where the snow resists dissolution the longest; at the bottom of declivities covered with snow, because the red substances are carried and deposited there by the currents.

Do the chronicles, manuscripts, or ancient works, make any mention of this snow?

I have never found any thing on this subject.

Does it exist on the Appennines as well as the Alps?

I have never thought of inquiring.

Is it observed rather on one side of the Alps than on the other?

It is found on one side as well as on the other.

Has any body ever seen it fall red?

Never; not even old people.

I have seen the same effects on snow occasioned by substances of another colour; these consisted of little particles conveyed by the winds, which had been taken up from the ground, or rocks, in the neighbourhood, which were of clay or plumbago; then the snow was black, and presented the same phenomena as the red. I remember to have seen somewhere that an author ascribed the colour of the red snow to the beams of the sun combined with it. But why then should not snow be every where red, since the sun-beams fall on one mountain as well as on another? It is to be observed that sometimes the edges of the snow which are most in contact with the ground are also coloured in the same manner. There are pretty generally in our mountains soils ferruginous by pyrites. There are even spongy slates (*ardoises spongieuses*) which contain pyrites, which have left vacancies by their solution, and which, being carried away by the water that flows

over the snow, may very well colour it. A league from our hospital, at the summit of the Col Ferret, there is a mine of specular magnetic iron ore (*fer spéculaire aimanté*); it may be recognized in spring by the snow which is strongly tinged with red.

<div align="center">I am, &c. BISELX.</div>

St. Bernard, March 6, 1819.

P. S. As soon as red snow can be had, I will filter some, and send you the residue; but I shall not find any before the middle of June.

Analysis of two Samples of Red Snow of St. Bernard. By M. Peschier, Member of the Physical and Natural History Society at Geneva. Extracted from a Memoir on the Subject, read to the Society.

I am not acquainted with any chemical inquiry into the cause of the colour of the red snow of the Alps, except that made by our illustrious countryman in 1778, and which is related in the third volume of his Travels. The results of this inquiry are confined to showing, that the residue of the red snow had an earthy appearance, that when laid on burning coals, it emitted a smoke, smelling like burnt grass; that it furnished a dark-brown solution, with muriatic acid, by the aid of heat, and a tincture of a beautiful gold-yellow with alcohol, which left, as the residue of its distillation, an oily substance, of a yellow-brown, having, while burning, the smell of wax, and that the loss in weight of the residue in this operation was $\frac{7}{40}$ths, which had made him at first consider it as a dust of stamina, and that microscopical observations had left him in doubt. As I was not acquainted with any thing else on the subject, I have always wished to see the attention of chemists directed to this interesting phenomenon.

Two favourable opportunities having offered, I have thought it my duty to take advantage of them; and the following are the essential parts of my inquiry:

My first operations were upon two residues of red snow, collected with great care by the Prior of Great St. Bernard. One of these residues, marked No. I. had an earthy appearance, and a ferruginous, dirty-yellow colour. No. II. had the character of a coarse vegetable earth, in which the naked eye could distinguish fragments of lichen and of moss. It came from a small spot of red snow, above which there was a reddish tinge, supposed by the Prior to be produced by a cryptogamous plant, which assumes this colour as it putrefies. This cause, he says, in the note which accompanies these residues, rarely occurs, and does not offer large coloured surfaces. No. I. when strongly heated, lost 0·10 of its weight, and assumed a darker colour. No. II. emitted a pretty considerable smoke smelling like burnt grass; lost 0·40, and left a brilliant residue, of a violet colour.

<div align="center">2 D 2</div>

'Subjected to the action of boiling alcohol, No. II. experienced only a very slight effect : cold and warm water did not produce any more.

Fifty grains of No. II. when submitted to destructive distillation, yielded an aqueous ammoniacal liquor, some drops of empyreumatic oil; and left a coaly residue, weighing 32 grs.

No. I. not containing any combustible substances, was not subjected to the same trial.

One hundred grains of No. I. experienced but slightly the action of muriatic acid, even with the aid of heat. The insoluble part, treated with nitric acid and an addition of sugar, furnished by means of a long ebullition, a solution which had a strong orange tinge ; and left a residue, weighing 65·50 grains, composed of fragments of stones and of rock crystal. The acid solutions furnished alumina 6·35, peroxide of iron 21·35 ; and there remained in solution in the pure alkaline liquor employed in the separation of the alumina, a vegetable principle, which had been dissolved by the acid, and which communicated to it a strong yellow colour. Its products are :

Siliceous substance .... 65·50
Alumina. .... 6·35
Peroxide of iron .... 21·35
Soluble organic substance .. 6·80
——
100·00

One hundred grains of No. II. furnished, with muriatic acid, a violet-brown solution, such as M. de Saussure obtained; but the action of this acid being found too weak to dissolve the oxide of iron, it was necessary to follow the same process as in the preceding operation, and the principles recognized were :

Insoluble substance .... 20·00
Alumina. .... 4·25
Peroxide of iron. .... 31·25
Chalk. .... 0·50
Insoluble organic substance.. 37·50
Ditto soluble as in No. I ... 6·50
——
100·00

A short time after this, having received from the Prior, by the medium of Professor Pictet, two bottles of water of red snow, with all the substances met with on its surface, the following experiments were made, which seem to throw a much greater light on the cause of this colour.

One of these bottles, No. I. contained 27 ounces of water ; it came, as the Prior expressed himself, from a snow which generally *covers* in June large tracts ; and which had a bright rose

colour, like lake; it had been covered with fresh snow at the time when it was collected, and did not show upon its surface any substance foreign to the colouring matter.   This snow had changed its colour in melting; the pieces visibly became of a fainter colour before they melted, merely by the transition to a higher degree of temperature.   It covers large spaces in June.

This water was colourless, slightly turbid, tasteless, had a smell analogous to that of a small quantity of decayed vegeto-animal substances; it furnished in this state a greenish liquid with hydrosulphuret of ammonia, and assumed, after the lapse of some hours, a violet tint with the infusion of galls: it did not affect the test papers; when filtrated, it experienced from the above tests the same effects: it was rendered slightly turbid by oxalate of ammonia, and gave no precipitate with salts of barytes: when exposed to the action of ebullition in an apparatus adapted to receive the carbonic acid gas, it gave but a very slight indication of it: when evaporated to dryness, it yielded a small deliquescent residuum, having the properties of extractive matter, and emitting on the coals a vegeto-animal smell.

The remainder left on the filter by this water, weighed 68 grs.; in this state it was externally of a greyish-violet, and internally it was of a very lively violet-red colour, which the action of the air soon changed to that of the surface.   It had penetrated into the substance of the paper, and was not to be separated from it without difficulty.   To the touch it was unctuous; it was pulverulent, and strewed with some thin filaments of vegetable substances.

When exposed to the action of alcohol with the aid of heat, it gave a tincture, of a deep purplish-yellow, and required several successive boilings with fresh alcohol before it ceased to colour it: the loss which it experienced by the solution of the colouring principle was found to be the same as that mentioned by M. de Saussure.

The spirituous liquids united yielded by distillation a colourless alcohol, having no extraneous taste, the last portions of which, being evaporated to dryness, left on the sides of the capsule a layer of a saffron-yellow, traversed by greenish dendritic ramifications.

This yellow substance had an acrid taste; when thrown on burning coals, it emitted a smoke that had a smell like burnt sugar, which was presumed to come from the alcohol: it was insoluble in water; but soluble in alcohol, ether, oil, the pure alkaline solutions, and chlorine; this last liquid destroyed its colour in dissolving it.   These properties, belonging to resinous substances, very well explain its nature.

Ten parts of the residue left by the water, strongly heated, emitted a copious smoke, with the smell of animal substances in combustion, and left a residue of a pale-red, with a loss of 3.

Twenty-five parts of this residue, treated by nitro-muriatic acid were found to be composed of

Siliceous substance	14·18	
Peroxide of iron.	3·25	The increase of the
Alumina	1·75	weight can only be
Chalk.	0·10	ascribed to the in-
Resinous principle.	3·20	terposed water.
Organic ditto.	2·25	
Ditto, ditto, soluble	1·75	
	26·48	

The water in the bottle No. II. was from a red snow, which is met with on the edge of the great masses of white snow; its colour was not so bright as that of No. I. and it did not change in melting.

This water, of which there was but a small quantity, was contained in a narrow bottle, the sixth part of which was filled with a brown and heavy deposit: when filtered, it retained a bright yellow colour, and left a residue, weighing, when dry, 48 grs.

This water stained the test papers red, by the effect of the carbonic acid; it gave, with the above-mentioned tests, liquids more coloured than that of No. I. and the oxalate of ammonia acted more powerfully upon it.

The residue had a brown tint; it was rough to the touch, and sprinkled with small fragments of rock crystal: alcohol and water had no sensible effect on it; the first furnished by its evaporation a small quantity of a yellow tincture; exposed to a brisk heat, it emitted neither smoke nor smell; treated by nitro-muriatic acid, 25 parts were composed of

Silex	1·25
Peroxide of iron	12·34
Chalk	0·20
Organic substance and water.	10·00
	23·79

The results of these different analyses seem to indicate that the red colour of the snow found in summer on some elevated parts of the Alps, arises from two different causes; viz. first, from a greater or less quantity of oxide of iron spread over its surface in a very great state of division, and in a very high degree of oxidation; secondly, from a resinous vegetable principle, of an orange-red colour, belonging, according to all appearance, to some cryptogamous plant of the genus alga, or lichen. And as nature presents us with a very great number of vegetables in which iron exists in pretty large quantity, it does not seem to be an inadmissible supposition, that this iron formed, perhaps, one of the immediate principles of the vegetables in question, of which

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Temperature

Chart of the Weather from Observations made at Manchester, in January, 1820,

by Thos. Hanson Surgeon.
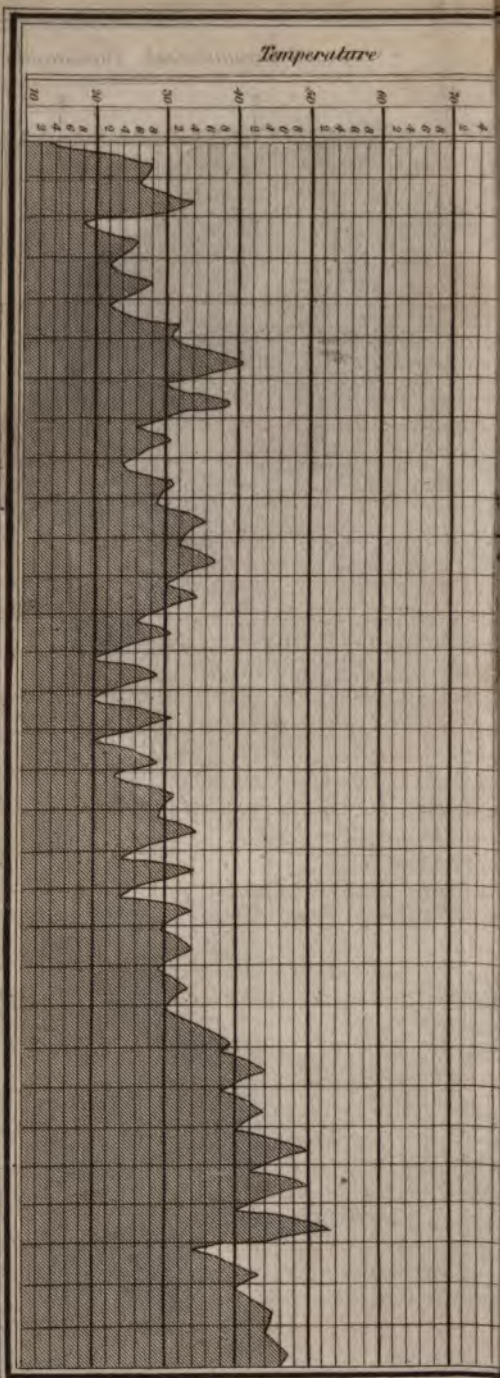
only the fragments are found, and that, in conjunction with the resinous principle, it is the direct cause of the colour. The proportions in which this metal is found, in these four analyses in 100 parts of the residue, are:

<div style="text-align:center">

Residue, No. I............ 21·35
———— No. II. .... 31·25
Water, No. I. .... 12·00
———— No. II............. 49·36

</div>

ARTICLE IV.

*Meteorological Journal kept at Manchester for 1819.
By Mr. T. Hanson. (With a Plate. See CV.)*

(To Dr. Thomson.)

SIR,   *Manchester, March 8, 1820.*

ENCLOSED I have sent you my annual results of the weather for the past year; also the results for the month of January, accompanied with a chart of the daily notations.

To the meteorologist, the chart scarcely needs any elucidation. The first and second horizontal spaces from the top are allotted for the days of the month and moon's age; the five following are for fog, rain, snow, hail, and thunder, as marked in the margin.

The spaces are darkened in proportion to the duration of the above occurrences, paying attention to the time of the day or night.

The course and strength of the wind by curves, with respect to the wind's force, 0, is considered a calm : 1, a gentle wind; 2, a little stronger; 3, a strong wind; and 4, a boisterous wind, or a hurricane.

With respect to the barometrical curve, I have depicted the real oscillations of the atmosphere as accurate as circumstances would allow.

The curve of temperature is formed from the daily extremes registered by a Six's thermometer. As the coldest part of the day is generally about an hour or two before sun-rise, and the warmest about two o'clock in the afternoon, those points in the chart denoting those periods of the day are carefully noted. Sometimes the extremes of temperature otherwise happen, particularly on the breaking up of a frost, or the setting in of one; when that is the case, the time is accordingly noted.

The left side of each daily perpendicular space denotes the morning, and the right the evening.

<div style="text-align:right">

I am, Sir, your most obedient servant,
THOMAS HANSON.

</div>

Meteorological Results of the Atmospherical Pressure and Temperature, Rain, Wind, and Clouds, deduced from diurnal Observations made at Manchester, in the Year 1819. By Thomas Hanson, Surgeon.

Latitude 53° 25′ North; Longitude 2° 10′ West of London.

1819. Months.	BAROMETRICAL PRESSURE							TEMPERATURE					RAIN		WINDS												CLOUDS						
	Mean.	Maximum.	Minimum.	Range.	Greatest variation in 24 hours.	Spaces in inches.	Numb. of changes.	Mean.	Maximum.	Minimum.	Range.	Greatest variation in 24 hours.	Inches.	Wet days.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.	Calm.	Brisk.	Boisterous.	Cirrus.	Cumulus.	Stratus.	Cirrocum.	Cirrostratus.	Cumulostratus.	Nimbus.
Jan.	29.52	30.34	28.76	1.58	0.56	3.95	8	41.8	62	33	29	23	4.060	22	0	0	3	5	0	12	3	4	3	0	3	3	0	21	1	6	0	3	0
Feb.	29.43	29.92	28.93	0.79	0.79	3.40	15	41.4	53	28	25	15	4.245	21	1	0	0	1	2	19	0	5	1	0	1	0	0	18	0	8	1	1	0
March	29.70	30.14	29.12	1.02	0.62	3.40	11	43.5	62	32	30	17	1.970	15	0	4	3	0	3	14	4	3	0	0	0	0	0	12	0	8	0	1	0
April	29.66	30.06	29.08	0.98	0.50	3.40	11	50.5	66	36	30	20	2.160	20	0	1	2	8	1	0	1	2	2	0	2	0	1	7	0	18	0	7	1
May	29.77	30.00	29.44	0.56	0.26	1.80	7	56.2	74	36	38	22	0.931	17	0	0	1	5	1	19	2	0	1	0	0	0	0	9	0	9	0	11	2
June	29.75	30.14	29.40	0.74	0.30	2.50	8	59.5	73	45	28	24	3.516	23	1	2	0	1	5	1	1	16	0	0	0	0	0	8	0	15	0	25	2
July	29.88	30.14	29.21	0.90	0.30	2.60	10	64.2	80	50	30	22	2.580	13	0	2	0	0	1	20	2	4	0	0	0	0	0	6	5	8	0	8	0
Aug.	29.31	30.34	28.98	1.26	0.36	2.20	6	65.8	79	50	29	22	1.939	12	0	0	3	1	3	3	3	4	1	0	1	0	4	8	3	2	4	8	1
Sept.	29.81	30.34	29.10	1.24	0.54	3.30	7	57.1	73	39	34	22	1.510	18	2	2	1	0	6	6	2	6	4	0	0	0	6	15	1	13	9	9	0
Oct.	29.67	30.15	29.20	0.95	0.41	3.10	10	50.1	70	30	31	21	4.917	21	0	5	2	1	2	7	3	6	2	0	0	0	0	19	0	7	0	6	0
Nov.	29.57	30.01	29.00	1.04	0.72	4.60	6	40.6	55	24	31	16	2.016	20	0	2	1	1	7	9	0	6	1	3	0	0	0	17	0	0	0	0	0
Dec.	29.57	30.01	29.17	0.87	0.64	3.80	12	55.5	56	20	36	13	5.152	17	0	2	5	6	7	9	0	0	2	3	0	0	0	17	0	0	0	0	0
Annual Means, &c.	29.08					37.15	116	50.7					35.019	215	4	21	21	35	44	124	21	53	16	3	9	55							

The annual mean pressure of the atmosphere is 29·68 inches; highest, 30·34, which was on Jan. 1, and on Sept. 21; lowest, 28·76, which occurred on Jan. 15; difference of these extremes, 1·58 inch, which is considerably below the usual range. The mean daily curve of the atmospherical oscillations, as shown by the barometrical surface, measures a little more than 37 inches. Total number of changes, 116.

The annual mean temperature is nearly 51°; the mean of the first three months, 42·9°; second, 55·4°; third, 62·4°; fourth, 42·1°: of the six winter months, 42·5°; six summer months, 58·9°. The maximum of 80° occurred on July 30; and the minimum of 20° on Dec. 10. Difference of the extremes, 60°.

The fall of rain, hail, snow, and sleet, which have fallen in and about Manchester is a trifle more than 35 inches. The greatest quantities of rain fell in January, February, October, and December; and the least in May. The notations in the above rain column for November and December have been furnished me by my friend Mr. John Dalton. My observations in those months were incorrect, in consequence of the frost breaking the bottle in the early part of November. A similar accident happened in December. Mr. Dalton makes the annual fall of rain, &c. 35·240 inches. Total number of wet days on which rain fell more or less, 215; out of this number, 48 may be designated completely wet.

The south-west, south-east, and north-west, have been the prevailing winds. Strong or boisterous winds have rarely occurred; out of nine instances on which brisk winds were noted, eight blew in the first four months of the year. The only boisterous winds of the year occurred in January, viz. on the 17th, 18th, and 25th, from the west and south-west. Snow has fallen on 25 days, and hail on 14 days.

The reporter has only noted five instances of thunder, viz. one in May, two in June, one in July, and one in October.

The temperature of the past year has been generally above an average, although not marked with any great elevations or depressions. The following will show the comparative difference between the monthly means of the past year, and the general monthly means of the 12 preceding years.

	Jan.	Feb.	Mar.	Apr.	May
Twelve years' general monthly mean	36·0	40·0	41·5	46·4	53·5
Monthly means in 1819.	41·8	41·1	45·5	50·5	56·2
Difference	p.5·2	p.1·4	p.4·0	p.4·1	p.2·7

June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
58·5	61·4	60·0	56·0	50·0	42·6	37·6
59·5	64·2	65·8	57·1	50·1	40·6	35·5
p.1·0	p.2·8	p.5·8	p.1·1	p.1·0	m.2·0	m.2·1

General annual mean temperature upon the 12 years, 48·7. Annual mean of the past year, 50·7; difference 2° above the general mean. From the above it appears, that the temperature of 1819 has been uniformly above the general temperature, except in November and December. The greatest differences were in January, March, April, and August. My friend Mr. Edward Stelfox, of Lymn, near Warrington, has favoured me with his account of rain. Mr. S.'s rain-gauge is exactly the same as mine, and I can rely upon his account as correct: his farm adjoins the rivers Mersey and Bollin, and is very much subject to be flooded. In January there fell 3·224 inches; February, 3·013; March, 1·352; April, 1·988; May, 2·035; June, 2·641; July, 2·649; August, 1·497; September, 1·695; October, 3·030; November, 1·881; December, 4·300 inches: total, 29·305 inches. Mr. Stelfox says, that the snow which fell on the night of Dec. 29 and the following day measured eight inches in depth. He observed the temperature on the mornings of Dec. 10 and 13 to be 19°.

Results of the Weather. (See the accompanying chart.)

Barometrical Pressure.—Mean, 29·72; highest, 30·64; lowest, 28·60; range, 2·04; greatest variation in 24 hours, 1; mean daily spaces in inches, 5·5; number of changes, 8; real spaces in inches, 8·8; real number of changes, 20.

Temperature.—Mean, 32·6; highest, 53; lowest, 13; range, 40; greatest variation in 24 hours, 19.

Rain, &c.—1·075 inch; number of wet days, 7; foggy, 21; snowy, 14; haily, 0.

Winds.—North, 0; north-east, 2; east, 7; south-east, 6; south, 4; south-west, 5; west, 0; north-west, 2; variable, 5; calm, 0; brisk, 1; boisterous, 1.

The greatest daily variation of the barometer took place on the 19th, accompanied with fog, rain, and snow, and was preceded by a boisterous north-east wind, which drifted the falling snow very much. It was on this day that the lowest state of the pressure occurred.

The reporter has been particularly attentive to the barometrical oscillations during the above period. There have been 20 changes; that is, risings and falls; and the mercurial surface has risen and fallen nearly nine inches. The monthly mean is a little more than a general mean: the highest extreme was on the evening of the 8th, attended with an east wind and a little snow. The temperature during the first three weeks averaged 28·6°; fourth week, 40·5°. On the morning of the 1st, and during part of the forenoon, the temperature was 19° below freezing; and in some exposed situations, it is said to have been as low as 9°, or 22°, under freezing. On the breaking up of the frost about the 20th, the ice was noticed in some places about 18 inches thick; and where the ground was nearly free from

snow, the frost penetrated about 14 or 15 inches below the surface.

The weather was extremely foggy, and snow fell copiously, with a gradual rising of the barometer during the first week. The mean daily temperature generally fluctuated between 20° and 30° till the 20th, when there was a great rise as already observed in the fourth week. The maximum of 53° took place about two o'clock on the 28th; in the course of the following night, the temperature fell to 34°, which marked the greatest daily variation for the month.

The east and south-east have been the prevailing winds; the brisk winds were variable; and the boisterous one from the north-east.

ARTICLE V.

On the Ligature of the Carotid Arteries as a Remedy for Determination of Blood to the Head. By C. R. Young, M.D.

THE tying of the carotid arteries in order to moderate a determination of blood to the head seems at once so simple and obvious a remedy for this disorder, that I am inclined to think it must have suggested itself long ago to the faculty, and that some weighty reasons can be opposed against the employment of it; or that it has been already tried and rejected for special reasons. But as my reading and information are not sufficient to assure me of these facts, I venture to make the proposal with a due sense of my own ignorance and inexperience, and with every sentiment of respect and deference for the wisdom and knowledge of others.

There is, I believe, in all or most graminivorous animals, which are forced to procure their food by grazing with their heads much lower than the rest of their bodies, a piece of structure in those arteries which corresponds to the carotids in the human species, by which the determination of the blood to the heads of these animals is prevented; for considering the time which they employ in taking in their food, this affection could hardly fail to occur in them were no such apparatus furnished by nature. This consists in a convolution of the arteries, by which the impetus of the blood coming from the heart must be much broken. Now is it not reasonable to expect that if by any means we could imitate this device of nature, in cases of determination of blood to the head in the human species, that a beneficial result might be obtained? Let us suppose that one of the carotid arteries were tied (for as in many cases there is evidence of a much stronger determination to one hemisphere of the brain than to the

other, it might be sufficient to tie the artery of the affected side only), and that the blood was forced into a longer and more circuitous course by the anastomosing branches; the arteries and veins may have lost their natural tone and calibre, and eventually no doubt the anastomosing branches themselves would become expanded and enlarged so as to admit as much blood to the brain as formerly; yet the tortuous course of the arteries would probably much diminish the vis a tergo, and deaden the impulse of the blood coming from the heart; so that a powerfully co-operating remedy might be obtained in addition to those generally used.

Of late, the operation of tying the carotids for aneurism has been successfully employed. I have not the advantage myself of knowing what phenomena have resulted from the operation with respect to the circulation of the blood in the head. As I believe it is a rule not to perform the operation until the anastomosing branches are sufficiently expanded to carry on the circulation, after the main artery has been closed, the case will not be in point; but probably much might be learned from an attentive observance of such cases. It is very rash to form *à priori* conclusions upon the effects of operations which have never been performed, more especially where so delicate and mysterious an organ as the brain is concerned. It is likely, however, that if both carotids were tied even where an inordinate distension of the blood-vessels of the brain had before occurred, that the flow of blood would at first be so much impeded that the patient might fall into the opposite extreme, and that fainting, or some affection arising from the presence of too small a quantity of blood in the head, would supervene. But this evil would only be temporary; and there are fortunately plenty of remedies for it much more efficacious than those now in use for the opposite affection. As most nervous diseases are supposed either to be connected with, or produced or aggravated by, a determination of blood to the head, I hope we may at length be enabled to discover some adequate means of subduing and keeping down these obstinate and untractable maladies.

ARTICLE VI.

Results of a Meteorological Journal for the Year 1819, kept at the Observatory of the Academy, Gosport.

By William Burney, LL.D.

Lat. 50° 47' 58" North. Long. 1° 6' 4" West of Greenwich. In time, 4' 24·3".

1819. Months	BAROMETER Maximum	Minimum	Medium	Range	No. of changes	Spaces described	Greatest variation in 24 hours	Medium at 8 a.m.	Medium at 2 p.m.	Medium at 8 p.m.	DAY & NIGHT THERM. Maximum	Minimum	Medium	Range	Greatest variation in 24 hours	Medium at 8 a.m.	Medium at 2 p.m.	Medium at 8 p.m.	DE LUC'S HYG. Maximum	Minimum	Mean range of the index	Medium at 8 a.m.	Medium at 2 p.m.	Medium at 8 p.m.	Mean at 8, 2, and 8 o'clock.
Jan	30·48	29·19	29·783	1·29	37	8·43	0·82	29·81	29·81	29·77	55	28	42·7	25·0	17	40·50	46·70	43·00	93	56	37·0	84·1	77·4	81·6	81·0
Feb	30·12	29·13	29·683	0·99	26	9·30	0·83	29·68	29·67	29·69	53	27	42·7	26·0	19	39·53	46·90	42·07	97	55	42·0	82·0	74·0	81·8	79·2
March	30·33	29·14	29·921	1·19	16	4·56	0·65	29·90	29·92	29·94	50	35	46·4	25·0	21	44·92	51·09	41·48	97	45	52·0	76·4	65·9	74·4	72·2
April	30·24	29·09	29·795	1·15	15	5·38	0·56	29·79	29·80	29·79	65	38	50·7	27·0	24	49·50	56·60	47·60	97	41	56·0	76·8	69·4	77·9	74·7
May	30·24	29·46	29·917	0·78	28	3·92	0·38	29·91	29·92	29·92	79	38	57·4	35·0	26	56·77	64·20	53·16	92	45	47·0	69·7	60·0	69·3	66·3
June	30·28	29·60	29·973	0·68	19	3·72	0·41	29·94	29·91	29·95	79	44	60·4	35·0	26	60·96	68·23	58·00	96	54	42·0	70·7	63·6	74·2	69·5
July	30·26	29·57	30·047	0·69	22	3·89	0·47	30·04	30·04	30·05	83	47	65·3	36·0	28	64·52	72·09	63·77	100	57	43·0	75·9	70·9	78·5	75·1
August	30·36	29·23	30·051	1·13	23	2·90	0·48	30·06	30·05	30·04	89	46	67·0	36·0	25	65·39	74·61	67·45	95	44	54·0	69·5	57·0	73·6	68·7
Sept	30·30	29·36	30·096	0·94	25	4·75	0·34	30·00	30·02	30·02	76	41	60·7	35·0	23	59·07	67·03	58·80	100	39	61·0	74·9	56·8	72·1	67·9
October	30·40	29·36	29·852	1·04	22	5·00	0·53	29·84	29·85	29·86	57	32	51·5	39·0	21	49·93	56·68	50·00	98	49	49·0	75·4	67·5	78·1	73·6
Nov	30·05	29·18	29·729	0·87	21	6·84	0·60	29·73	29·73	29·72	51	26	42·7	31·0	20	39·97	46·23	42·40	100	60	40·0	77·6	73·5	79·0	76·7
Dec	30·20	29·22	29·724	0·98	24	6·50	0·58	29·72	29·71	29·72	53	15	37·8	38·0	21	36·00	41·06	37·74	100	54	46·0	82·7	77·9	84·6	81·7
	30·50	29·09	29·881	11·93	280	65·19	0·83	29·87	29·87	29·87	83	15	52·10	32·3	30	50·53	57·62	50·84	100	39	47·4	76·3	67·8	77·1	73·7
Results for 1818.	30·60	28·50	29·881	12·59	274	78·60	1·00	29·87	29·87	29·88	91	23	52·79	32·8	32	51·80	59·30	51·42	100	29	48·0	69·0	61·6	69·7	66·8

Months (1819)	Rain, hail, snow, and sleet, in inches, &c.	Evaporation in inches, &c.	Thunder	Lightning	Small meteors	Rainbows	Lunar halos	Solar halos	Paraselenæ	Parhelia	Anthelia	Rain, &c.	Fog	An overcast sky	Fair, with clouds	A clear sky	Nimbus	Cumulostratus	Cumulus	Stratus	Cirrostratus	Cirrocumulus	Cirrus	Total number of days	North-west	West	South-west	South	South-east	East	North-east	North
Jan.	4·17	0·55	2	2	2	1	2	2	2	12	1	12½	1	4	11	2	20	15	9	2	23	8	9	31	3½	8	5½	7½	3½	1½	4½	1
Feb.	3·33	1·00	0	0	1	3	0	3	0	1	0	10	—	5	10½	2	22	20	23	7	26	20	16	28	5	9	3½	4	—	6	—	5
March	1·18	1·70	0	0	6	2	4	3	0	1	0	9	0	10	11	1	15	21	14	5	28	13	15	31	4	8	9	—	3½	9	3	5
April	3·55	2·55	0	1	0	4	2	4	0	5	0	9	0	6	12	4	17	16	21	9	21	18	20	30	—	8	9	3½	9	5½	1	2
May	3·80	3·45	1	3	0	1	4	6	0	4	1	7½	0	6	14	2	18	17	24	2	30	18	25	31	3½	4	—	2	—	0	2	3½
June	1·86	4·55	5	5	1	3	0	2	0	4	3	10½	0	5½	15	2½	10	18	24	0	29	20	21	30	7	2	3	2	1	5½	3½	4
July	2·00	4·45	0	0	29	2	0	4	0	0	2	5	0	5	18	5	7	15	25	3	27	23	33	31	6½	5	11½	4	2	4½	4	6
August	0·56	5·15	3	3	47	1	0	2	0	0	2	8	0	4	15	3	15	12	21	1	24	24	23	31	5	6½	11½	2½	2½	4	3	4
Sept.	2·86	3·90	0	0	19	0	3	2	2	1	0	6	0	4	17	4	11	14	16	3	21	18	20	30	3½	7	5	1	2	5	3	2½
Oct.	2·67	2·02	0	0	9	1	1	1	0	5	0	9½	6½	6½	10	9	18	9	15	5	20	18	14	31	7	5	9	2½	2½	7½	2	6½
Nov.	3·44	1·14	0	0	3	0	1	3	2	6	5	—	—	6	12	3	19	16	8	6	16	10	11	30	9	6	2½	1	—	—	—	4
Dec.	3·90	0·80	0	0	4	0	8	2	1	—	—	10	0	6	12	3	19	16	9	6	16	9	14	31	5	8½	2½	2	—	7½	1	2
Results for 1818.	33·33	31·26	12	16	121	18	25	34	7	7·58	7·58	99	2¼	66	164	39½	189	188	215	39	294	199	209	365	60	87½	34½	35	29	49½	24	45½
1818.	27·94	49·80	15	15	31	10	23	24	8	3·26	3·26	101	6½	98	178	41½	160	100	133	22	200	100	194	365	43	68	62	54½	26½	47½	38	25½

ANNUAL RESULTS.

Barometer.

Inches.

Greatest barometric pressure, Sept. 21.   Wind, N.E. ... 30·50
Least ditto, April 16.   Wind, S...................... 29·09
Extreme range of the mercury. .... 1·41
Annual mean barometric pressure .... 29·881
Ditto ditto at 8, a. m. .... 29·870
Ditto ditto at 2, p. m. .... 29·871
Ditto ditto at 8 p. m............................. 29·874
Mean pressure for 190 days, with the moon in N. declination 29·911
Mean pressure for 175 days, with the moon in S. declination 29·851
Greatest range of the mercury in January .... 1·290
Least range of the mercury in June.................. 0·680
Greatest variation in 24 hours in February............ 0·830
Least variation in 24 hours in September............. 0·340
Spaces described by the rising and falling of the mercury 65·190
Number of changes caused by the variations in the weight
    of the atmospheric column 280

Thermometer.

Greatest thermometrical heat, July 31.   Wind, N.E. ... 83·00°
Greatest cold, Dec. 29.   Wind, N.................... 15·00
Extreme range of the thermometer................... 68·00
Annual mean temperature of the atmosphere.......... 52·10
Ditto ditto at 8 a. m. .... 50·53
Ditto ditto at 2 p. m............................. 57·62
Ditto ditto at 8 p. m............................. 50·84
Greatest range in October. .... 39·00
Least range in January and March.................. 25·00
Annual mean range .... 32·33
Greatest variation in 24 hours in June. .... 30·00
Least variation in 24 hours in January. .... 17·00

De Luc's Whalebone Hygrometer.

Greatest humidity of the atmosphere several times. .... 100·0°
Greatest dryness of ditto, Sept. 1.   Wind, N.W. ...... 39·0
Extreme range of the index........................ 61·0
Annual mean state of the hygrometer at 8 a. m. .... 76·3
Ditto ditto at 8 p. m............................. 77·1
Ditto ditto at 2 p. m............................. 67·8
Ditto ditto at 8, 2, and 8 o'clock .... 73·7
Greatest mean humidity of the atmosphere in Dec. .... 81·7
Greatest mean dryness of the atmosphere in May ...... 66·3

Prevailing Winds

	Days.
From north to north-east.	45¼
From north-east to east.	24
From east to south-east.	49¼
From south-east to south.............................	29
From south to south-west............................	35
From south-west to west.............................	34½
From west to north-west............................	87¼
From north-west to north.	60
	——
	365

Clouds, agreeably to the Nomenclature.

	Days.
Cirrus	209
Cirrocumulus	199
Cirrostratus.......................................	294
Stratus	39
Cumulus.	215
Cumulostratus	188
Nimbus..	189

A Summary of the Weather.

	Days.
A transparent atmosphere, without clouds	33¼
Fair, with various modifications of clouds..............	164
An overcast sky, without rain.......................	66
Fog...	2½
Rain, hail, sleet, and snow..........................	99
	——
	365

Atmospheric Phenomena.

	Number.
Anthelia, or mock-suns, nearly opposite to the real sun. ...	7
Parhelia, or mock-suns.	58
Paraselenæ, or mock-moons.	7
Solar halos.......................................	32
Lunar halos	25
Rainbows, perfect	18
Small meteors, commonly called shooting stars	121
Aurora Borealis, Oct. 17, in the evening.	1
Lightning, days on which it has occurred	16
Thunder, ditto ditto	12

Evaporation.

	Inches.
Greatest quantity in August.........................	5·15
Least quantity in January	0·55
Total quantity for the year	31·26

Rain, &c.

	Inches.
Greatest quantity in January	4·17
Least quantity in August	0·56
Total quantity for the year	33·33

N.B. The barometer is hung up in the observatory, about 30 feet above high-water mark; and the self-registering horizontal day and night thermometer, and De Luc's whalebone hygrometer, are placed in open-worked cases near a wall, in a northern aspect, out of the sun's rays, and 10 feet above the level of the garden. The pluviameter and evaporator have respectively an area of six inches square: the former is emptied every morning at eight, a. m. after rain has fallen into a cylindrical glass gauge accurately graduated to $\frac{1}{100}$th part of an inch; and the quantity evaporated from the latter is ascertained every third day.

Barometric Pressure.—Thinking the barometer would stand higher while the moon was in north declination than when she was in south declination, we separated the time, and found the mean barometric pressure during her north declination this year in favour of our conjecture: but as the difference, while in the northern part of her orbit, amounts only to $\frac{1}{17}$th of an inch, we are therefore inclined to think that, *in a local point of view,* nothing satisfactory to meteorology can be drawn from this experiment; although it has recently been suspected by meteorologists that the moon has much more influence on the earth's atmosphere in one part of her orbit than in the other. If future results should be found to decide the question, they must, we think, accrue from a series of combined barometric observations in many distant places both in north and south latitude. But until observations of this sort be more generally and very regularly made in many favourable places, it is probable that the question will remain unsolved. In observations that may be made to determine it, the precise state of the winds, morning, afternoon and night, should be particularly registered, and from a high vane, as a great deal will depend upon the prevalence of westerly and southerly winds, which are found to diminish the weight of the atmospheric column, and consequently shorten the barometric or mercurial column, in whatever part of her orbit the moon may be in at the time. The annual mean barometric pressure this year agrees exactly with that for the preceding year, as shown in the table.

Temperature.—The mean temperature of Gosport for the last two prolific years is $1\frac{9}{10}°$ more than in 1817; but the *maximum* and *minimum* temperatures for 1819 are respectively 8° lower than that of 1818. The mean of the observations taken at eight, a. m. and eight, p. m. is nearly 1°, and the mean of those taken at two, p. m. 1·68° less this year than last; yet there is a near-

coincidence between the means at eight, a.m. and eight, p.m. for either, or for both the years together. The same may be said of the mean barometric pressure at those times.

 Humidity of the Atmosphere.—By accurate observations made with De Luc's whalebone hygrometer, the mean diurnal humidity of the ambient air is nearly 7° greater this year than the preceding; but it will be recollected that the latter part of the spring, and both the summer and autumn of 1818 were unusually dry and warm; so much so that the evaporation was one-third more, the depth of rain one-third less, and the modifications of clouds less frequent in their appearance *that* year than this—these circumstances combine in establishing the fact of a more vaporous atmosphere, agreeably to the indications of the instrument.

 Prevailing Winds.—The variableness of the winds during 1818 and 1819 may be seen in that part of the table containing the results of the prevailing winds; but when we compare the number of days they have come from the east of our meridian, with the number from the west of it, the difference in the two years' results is only a few days. The winds to the eastward of the meridian this year have prevailed 148 days, and those to the westward of it 217 days; difference in favour of the latter 69 days; and this difference agrees with that of last year within one day—to this may be attributed the coincidence of the mean barometric pressure. The number of strong gales of wind from the following points of the compass, or rather the days they have prevailed, are 34; namely, N. 3, N.E. 1, E. 5, S.E. 0, S. 4, S.W. 9, W. 9, and N.W. 3.—There being much doubt as to the accuracy of registering the winds from the positions of low vanes, in consequence of the eddy and baffling winds that frequently prevail with the land breezes, from attractions and obstructions on and near the earth, we therefore register from a high vane, and sometimes from the direct motions of the lower clouds. The propriety of this method we urged in our last Annual Meteorological Results, published in the *Annals of Philosophy.* The state of the winds this year, as well as last, was drawn up from three observations each day, as well as from frequent observations in the nights, according to the precise duration of each wind.

 Clouds.—The last table contains the number of days on which the various modifications of clouds have appeared, and some of them by night as well as by day. The *cirrostratus* cloud, in consequence of the humidity of the atmosphere, has appeared most; and the *stratus*, which is confined in its appearance chiefly to autumn, the least, as is usual. The *cumulostratus* and the *nimbus* are nearly even in numbers; and also the *cirrus* and *cumulus.* The *cirrocumulus* cloud, whether in elevated round flocks, or attenuated beds, is characteristic of a rising temperature soon after its appearance, but mostly on the subsequent day. The *cirrus* and *cirrostratus* presage a falling temperature, rain,

and wind; but the *stratus* and *cumulus* generally indicate fair weather for the time of their appearance. The *cumulostratus*, even by inosculation, seldom passes to a *nimbus*, or rain cloud, unless an extensive *cirrus* should happen to unite with it in its descent. The *cumulus*, whose appearance is confined to the day, is formed by a collection of vapours rising out of marshes, rivers, &c.; it generally rises in an hemispheric form, moves in the direction of the wind, and often evaporates perceptibly after sunset in calm weather. It is probable that the nascent *cumulus* is sometimes formed by the rarefaction of the *stratus* in fair weather. The lofty *cumulus* too has often been seen to transform itself into a *cirrocumulus*. The *cirrostratus* is formed by descending *cirrus*, or *cirrocumulus*, and sometimes comes down so low as to sweep the surrounding hills: when stationary and attenuated, it often resolves into dew after sunset. Such is our short but practical view of the nomenclature of clouds.

Weather.—The most remarkable difference of the two years' results under this head is in the number of completely overcast days and nights, being 28 more this year than last; and the clear or cloudless days and nights are eight less. Here it is necessary to explain the method by which the exact number of days, under the respective divisions of weather, was ascertained. It was done thus: if a cloudless day happened, and the sky was not clear, or free from clouds at night, only half the day was accounted for as being clear; the same regard to time was paid to the continuance of rain, &c. If the day happened to be fair, with sunshine and clouds, and the sky overcast at night, the time was equally divided under those heads.

Atmospheric Phenomena.—The most striking difference in the results of the atmospheric *phenomena* for the last two years, is in the number of meteors (some of them of a large size); of 121, no less than 95 appeared in the evenings of July, August, and September, the three hottest months in this year. It may, therefore, be inferred, that they are generated by heat in an atmosphere highly charged with electric matter. There is, however, a material difference in the quality of meteors, which may be determined chiefly by their colours, apparent densities, and altitudes.

Evaporation.—The quantity evaporated this year is not so great as it was last by $18\frac{1}{4}$ inches; nor does it amount to so much as the depth of rain, &c. by 2·07 inches; consequently the ground at the close of the year must have been in a moist state.

Variation of the Magnetic Needle.—From the mean of daily observations on the magnetic needle this year, it has been found to decrease about 2′ in its western course, compared with observations made last year; but whether this recession will be progressive is a question of no small importance, and which must be

decided by further observations; if so, the magnetic needle may
be said to have arrived at its *maximum* variation westward. The
mean variation of the magnetic needle at the close of 1819 was
24° 36¼′ W.

ARTICLE VII.

*Description of an Urinary Calculus, composed of the Lithate or
Urate of Ammonia.* By William Prout, M.D. F.R.S.*

M. FOURCROY had stated that the lithate of ammonia not
only frequently enters into the composition of urinary calculi,
but sometimes constitutes entire concretions.† Mr. Brande,
some years afterwards, called this statement in question, and
was induced to conclude from his experiments, " that no sub-
stance which can be called urate of ammonia exists in calculi."‡
In this latter opinion I believe most British chemists have acqui-
esced, and Dr. Marcet, in his recent work on this subject,
observes, " the presence of this substance (lithate of ammonia)
in urinary calculi I still think very doubtful, especially because,
since it is so easily discoverable in the excrements of the boa
constrictor, it is not probable that the English chemists would
have overlooked it so long in the human calculi, which they have
so often and so successfully submitted to chemical examination."§
From these decided opinions of such eminent chemists, we must
conclude that this variety of calculus is extremely rare: to obviate,
however, the belief that it does not exist at all, I have been
induced to draw up the present account, the object of which is
to describe a calculus composed almost entirely of the substance
in question.

This calculus, for which I am indebted to my friend Dr. Elliot-
son, was extracted in April last by Mr. Cline, jun. from a boy
about two years of age, in St. Thomas's Hospital : when entire,
it weighed about 50 grs.; its general shape was ovoid a little
flattened ; its external surface was smooth, and of a greenish-
clay colour (corresponding nearly to the *wax-yellow* of Werner.‖)
It was composed of thin concentric layers, easily separable from
one another, and readily breaking into sharp angular pieces, with
a compact earthy fracture. Its general colour internally differed
both in shade and intensity from that of its external surface : it
might be denominated a pale reddish clay colour (corresponding

* From the Medico-chirurgical Transactions, vol. x. p. 389.
† Système des Connaissances Chimiques, tom. x. p. 224.
‡ Phil. Trans. vol. xcviii. p. 231.
§ Essay on the Chemical History and Medical Treatment of Calculous Disor-
ders, p. 140, first edition. See also Dr. Henry's paper in the present volume, p. 107.
‖ See Werner's Nomenclature of Colours, by Patrick Syme.

nearly to the *wood-brown* of Werner.*)   The different layers, however, differed somewhat in intensity, which caused the laminated structure to be visible to the eye.   Between some of the layers also there were minute depositions of the earthy phosphates, which render this structure still more sensible.   The nucleus exhibited the same general appearance as the rest of the calculus, except that it appeared to be made up of a fine powder and a few larger grains, loosely agglutinated together.

It was sparingly soluble in cold water,† but it dissolved readily in boiling water (especially when in a state of fine powder), requiring only about 300 times its weight for that purpose.   On cooling, the calculous matter did not immediately separate, but after some days a great part of it was deposited.

It readily dissolved in solutions of the fixed alkalies, and at the same time a strong smell of ammonia was exhaled.   When muriatic acid was added to this solution, lithic acid was precipitated.

In nitric acid it dissolved readily, especially with the assistance of heat, exhibiting the same phenomena as lithic acid when similarly treated.

Muriatic acid, in which it had been digested, was found to be converted into muriate of ammonia.

Exposed to the action of heat by means of the blow-pipe, it decrepitated so strongly that it was difficult to ascertain the effects produced by this agent.   When reduced to powder, and exposed to heat, it first appeared to give off ammonia, and afterwards to burn with the same phenomena as lithic acid.   It left a minute residuum, which strongly reddened turmeric paper, and appeared to consist partly of lime (and alkali), and partly of the earthy phosphates.

From these properties it is evident that this calculus consisted principally of the lithate of ammonia. ‡

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* See Werner's Nomenclature of Colours, by Patrick Syme.
† One part of the excrements of the boa constrictor (which is lithate of ammonia) at 60° required about 480 parts of water to dissolve it,

$$\text{At } 90°\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ 300$$
$$\text{At } 212°\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ 240$$

But the calculus above described was found to be somewhat less soluble than this substance, probably on account of its compact state of aggregation.

‡ The following is Fourcroy's description of this species of calculus, which does not differ much from the above.   " Les calculs d'urate d'ammoniaque, bien charactérisés par leur dissolubilité dans les lessives d'alcalis fixes caustiques, mais avec un dégagement abondant d'ammoniaque, sont ordinairement petits, d'une couleur pâle de café au lait, ou d'un gris tirant sur cette nuance, formés de couches fines qu'on détache facilement les unes des autres, et qui sont lisses par les surfaces qui se touchent ; presque toujours contenant un noyau dont on sépare aisément l'enveloppe.   Leur forme la plus ordinaire, est sphéroïdale, alongée, comprimée, quelquefois amygdaloïde ; leur surface est ordinairement lisse, jamais tuberculeuse, quelquefois brillante et crystalline ; leur pesanteur spécifique va de 1.225 à 1.720, l'eau seule les dissout, surtout quand elle est chaude, et quand ils sont divisés et en poussière fine.   Les acides, le muriatique surtout, leur enlèvent l'ammoniaque, et laissent seul l'acide urique, qui se dissout ensuite dans la potasse sans effervescence.

The boy from whom this calculus was taken suffered extreme irritation, and his general heath was much deranged. Two or three weeks before it was extracted, I had an opportunity of examining his urine ; it was pale-coloured, and exhibited the appearance it usually assumes when a calculus is present in the bladder, or when the functions of the inner coat of that viscus are otherwise deranged. Its specific gravity was 1023·8, and it abounded in urea and the triple phosphate of magnesia and ammonia. It reddened turmeric paper, but as it had been kept for some days before I had an opportunity of examining it, this property might have been acquired after it was voided from the bladder.

I possess a fragment of another small calculus, having precisely the same colour and properties as that above described. It was likewise taken from a boy under the age of puberty, and was accompanied by great irritation. This fragment, which is about one-tenth of an inch in thickness, appears to have constituted a part of the outer crust. Its external surface is rough, and covered with mamillary protuberances. To a part of its internal surface there is adhering a portion of a common lithic acid calculus ; probably, therefore, the whole of its centre was composed of that substance. This boy, as well as the former, recovered from the operation, and I believe neither has ever had any return of the complaint.

The characteristic properties of this species of calculus appear then to be the following : 1, their colour and general appearance, which are peculiar ; 2, their solubility in water ; 3, their yielding ammonia when treated with the fixed caustic alkalies. To which, perhaps, may be added, 4, their property of decrepitating before the blow-pipe.*

There are also strong reasons for concluding, from the smallness of their size, and other circumstances, that this species of calculus, in its pure state, is peculiar to children under puberty,† and that it is accompanied by great derangement of the general health, and the most distressing irritation.

With respect to the *medical treatment* of this variety of calculus, it ought probably to differ in no respect from that adopted in ordinary cases of the lithic acid calculus ; certainly not at least in a *chemical* point of view.

ils se trouvent quelquefois recouverts d'acide urique pur : la couche extérieure de celui-ci est ordinairement peu épaisse, et la plus grande quantité de calcul est de l'urate d'ammoniaque. Sur les 600 calculs examinés la proportion du nombre d'individus de cette espèce a été une des plus foibles." Op. cit. p. 237.

* I am aware that decrepitating calculi are usually said to contain a little oxalate of lime, and this was perhaps the case in both the above instances. In these instances, however, the decrepitation appeared to me rather to depend on the escape of ammonia.

† The morbid urine of children generally contains an excess of the phosphates, but in some rare instances a peculiar clay-coloured deposition takes place after the urine has cooled, which, if I am not mistaken, consists partly of lithate of ammonia.

ARTICLE VIII.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London,
for 1819, Part II.

THIS part contains the following papers :—

I. *On the Specific Gravities and Temperature of Sea Waters in different Parts of the Ocean, and in particular Seas; with some Account of their saline Contents.* By Alexander Marcet, M.D. F.R.S. &c.—Dr. Marcet had formed the project along with Mr. Smithson Tennant of collecting specimens of sea water from different seas, and at different depths. In the course of a few years a considerable number of specimens were collected, when Mr. Tennant's unfortunate death deprived Dr. Marcet of his assistance, and occasioned so much procrastination that the subject ran a considerable risk of being abandoned altogether, when our author's attention was again recalled to it by the late expedition in search of a north-west passage. The officers who had the charge of that expedition were very assiduous in collecting specimens of sea water, and in determining the temperature of the sea at the surface, and at the bottom, in various latitudes. With these specimens and results, they liberally supplied Dr. Marcet, and thus invited him to resume his investigations, and complete them. The consequence has been the production of the elaborate and valuable paper of which I shall endeavour to give an analysis.

The number of specimens of sea water collected amounted to 68. The following table exhibits the specific gravities of these specimens, together with the parts of the ocean from which they were collected :

	Nos.	Lat.		Long.		Sp. grav.	
	1	66° 50′		68° 30′ W		1·02555	From a depth of 80 fathoms.
	2	74 00		—		1·02546	From the surface.
	3	74 50		59 30		1·02619	
	4	75 14		4 49 E		1·02727	From the surface.
	5	75 14		4 49 E		1·02727	From a depth of 756 fathoms.
	6	75 54		65 32 W		1·0227 *	From the surface four miles from land.
	7	75 54		65 32		1·0259	From a depth of 80 fathoms.
Arctic	8	76 32		76 46		1·02405	From the surface.
Ocean.	9	76 32		76 46		1·02622	From a depth of 80 fathoms.
	10	76 33		—		1·02664	From a depth of 80 fathoms.
	11	79 57		11 15 E		1·0267	From a depth of 34 fathoms.
	12	80 26		10 30		1·02255 *	From the surface. Ship beset with ice.
	13	80 26		10 30		1·02714	From bottom; depth, 237 fathoms.
	14	80 26		10 30		1·02715	Ditto, ditto.
	15	80 28		10 20		1·0268	Ditto; depth, 183 fathoms.
	16	80 29		11 0		1·02684	Ditto; depth, 305 fathoms.

	Nos.	Lat.		Long.		Sp. grav.	
	17	63° 49'	55° 38' W			1·0267	From a depth of 80 fathoms.
	18	59 40	14 46			1·03004	From the surface.
	19	56 22	—			1·02656	
	20	54 00	4 30			1·0268	
	21	53 -45	0 20			1·0267	
	22	52 45	4 00			1·02175 *	From near the mouth of the river Mawdack.
Northern hemis-phere.	23	48 25	6 34			1·03002	
	24	46 00	48 00			1·02648	
	25	45 20	45 10			1·02816	From a depth of 250 fathoms.
	26	45 10	15 00			1·02934	
	27	25 30	32 30			1·02886	
	28	22 00	89 00 E			1·02028*	From the mouth of the Ganges.
	29	13 00	74 00			1·02772	
	30	10 50	24 26 W			1·02825	
	31	7 00	80 0 E			1·03009	
	32	4 00	23 0 W			1·02772	
	33	3 28	81 4 E			1·03022	
Equator.	34	0 00	25 30 W			1·02825	Temp. 82°.
	35	0 00	23 0			1·02785	
	36	0 00	83 0 E			1·02807	
	37	0 00	92 0			1·02692	
	38	8 30 S	32 0 W			1·02895	
	39	9 00	35 0			1·02920	
	40	11 30	33 7			1·02980	
Southern hemis-phere.	41	21 00	0 0			1·02819	
	42	23 30	73 0 E			1·02831	
	43	25 30	5 30			1·03209	
	44	28 00	43 0			1·02715	
	45	35 00 +	56 0 W			1·02545	Mouth of the Rio de la Plata.
	46	35 10	21 0 E			1·0275	
	47	35 33	0 21			1·0316	
Yellow Sea.	48	35 00+N	—			1·02291	
Mediter-ranean.	49	36 00+	5 0 W			1·0301	From a depth of 250 fathoms.
	50	36 00+	5 0			1·0305	From the surface.
	51	—	—			1·0273	From the sea at Marseilles.
Sea of Marmora.	52	40 5	26 12 E			1·02819	From the bottom, 34 fathoms deep.
	53	40 5	26 12			1·02028	Same spot, but from the surface.
	54	41 0+	29 00			1·01444	From bottom, 30 fathoms.
	55	41 0+	29 00			1·01328	Same spot, from the surface.
Black Sea.	56	—	—			1·01422	
	57	—	—			1·01414	
White Sea.	58	65 15	39 19			1·01894	
	59	—	—			1·01909	
Baltic.	60	56 00	15 00			1·0049	Carlsham Harbour.
	61	57 39	—			1·02593	Cattegat; depth, 14 fathoms.
	62	56 00 N	12 40 E			1·01587	From the passage into the Baltic.
Ice-sea waters.	63	75 54	65 32 W			1·00000	From the same spots as Nos. 6 and 7.
	64	80 28	10 20 E			1·00017	Water at the surface beset with ice.
	65	79 56	11 30			1·0006	From a floe.
	66	79 38	11 00			1·00015	From an iceberg.
	67	79 48	13 40			1·00235	From the surface of a piece of ice.
	68	75 40	61 20 W			1·00015	From young ice under the surface.

It would appear from the preceding table, that the ocean in . the southern hemisphere contains more salt than in the northern hemisphere in the proportion of 1·02919 to 1·02757 ; but the great proportion of the specimens in the northern ocean were taken at a much higher latitude than those in the southern.

This may partly account for the difference. In estimating the specific gravity of the water from the northern hemisphere, the specimens marked with an asterisk (*) have been omitted, as obviously below the truth, either from ice, or from the fresh water of rivers.

The mean specific gravity of water from the equator is 1·02777. This exceeds a little the mean gravity which prevails in the northern hemisphere; but falls short of that in the southern hemisphere. The mean specific gravity in the northern, southern hemispheres, and at the equator, is as follows:

Northern hemisphere .... 1·02757
Equator......................... 1·02777
Southern hemisphere .... 1·02919

But it ought to be remarked, that Dr. Davy generally found the specific gravity of sea water both in the South Atlantic and in the Indian Ocean less than what is stated in the preceding tables. When sea water is taken from the surface, it may be conceived to be modified a little by the quantity of rain which has fallen; for it is reasonable to believe that immediately after a heavy fall of rain, the specific gravity of water taken at the surface of the sea will be a little less than it would be if taken after a long tract of dry weather.

There is no satisfactory evidence that the sea at great depths is more strongly impregnated with salt than at the surface; except at the mouth of the Dardanelles where the specific gravity of the water at the surface was 1·02028, while the specific gravity of the water from the bottom was 1·02819. We may conceive this to be owing to a current of water from the Mediterranean flowing into the Black Sea at the bottom, while a counter current from the Black Sea flows towards the Mediterranean at the surface; for the specific gravity of the water at the bottom approaches that of the Mediterranean, while that at the surface approaches the specific gravity of the Black Sea.

The Mediterranean Sea contains more salt, and has a higher specific gravity than that of the Atlantic Ocean; while most other inland seas, as the Baltic and the Black Sea, contain less salt than the ocean. This is accounted for by supposing that the evaporation in that sea is greater than the supply of fresh water; the consequence of which is, that a current from the Atlantic sets into the Mediterranean through the straits of Gibraltar; and in order to obviate the supposed consequence, that on such an hypothesis the saltness of the Mediterranean would continually increase, it has been conceived that an under current of salter water sets in from the Mediterranean to the Atlantic at the bottom of the straits of Gibraltar.

Dr. Marcet is of opinion, that he is entitled to conclude from the specimens of water from ice taken from the sea which he has examined, that the ice of sea water is always perfectly fresh; but

I do not see that the specimens in question prove the truth of this opinion; for much of the ice that floats in the Arctic Ocean has been originally produced in rivers, or on the sea coast, and it has been afterwards fed by the snow and rain water which fell upon it while floating, and which would gradually assimilate itself to the parent stock. Water saturated with salt freezes at 4°, and the sea water on our coasts freezes at about 28°; but I have never been able to satisfy myself that in the act of freezing, it parts with the whole of the salt which it held in solution. This may, perhaps, be the case; but it is difficult to conceive how the particles of salt, which ought to be at least mechanically mixed among the crystals of ice, could make their escape from a very large mass of ice.

The temperature of sea water ought to diminish gradually from the surface to the bottom; because its density constantly increases as its temperature diminishes till it reaches, or even passes, the freezing point of sea water. This diminution of temperature was observed by Capt. Ross and Lieut. Parry to hold in Davis's Straits and in Baffin's Bay; but it was determined by Lieut. Franklin and Lieut. Beechy that on the east side of Greenland, and in the polar Seas, just the contrary takes place, or the sea at the bottom is considerably hotter than at the surface.

As it would have been an endless task to have attempted the analysis of all the specimens of sea water contained in the above table, Dr. Marcet selected 16, and determined with as much accuracy as possible the quantity of muriatic acid, sulphuric acid, lime, and magnesia, contained in 500 grs. of the liquids, and likewise the weight of salt left, when 500 grs. of each of the liquids was evaporated to dryness. The muriatic acid was estimated from the quantity of chloride of silver precipitated, and dried at a temperature sufficient to produce incipient fusion. The sulphuric acid by the quantity of sulphate of barytes. When 100 grs. of sulphate of barytes, dried at 212°, are exposed to a red heat, they are reduced to 97·2 grs. The lime was determined by precipitating it by oxalate of ammonia, and drying the precipitate at the temperature of 212°. According to Dr. Marcet, 100 grs. of such a precipitate contain 39·23 grs. of lime. The magnesia was precipitated by means of phosphate of soda and carbonate of ammonia. According to Dr. Marcet, 100 grs. of this precipitate contain 40 grs. of magnesia. The following table exhibits the weight of the saline contents, and of the different precipitates, from 500 grs. of the specimens of sea water subjected to experiment:

Specimen.	Spec. grav.	Salts from 500 grs. of water.	Muriate of silver.	Sulphate of barytes.	Oxalate of lime.	Phosphate of magnesia.	Total precipitate from 500 grs. of water.
		Grs.	Grs.	Grs.	Grs.	Grs.	Grs.
1	1·02727	19·5	39·7	3·3	0·85	2·7	46·55
12	1·0197	14·15	27·9	2·4	0·7	1·8	32·8
67	1·00235	1·75	3·2	0·1	0·05	0·3	3·37
14	1·02705	19·3	38·9	3·25	0·95	2·9	46·0
35	1·02785	19·6	40·3	3 7	0·9	3·1	48·0
41	1·0	20·6	40 4	3·75	1·0	3·2	48·3
58 and 59	1·0	16·1	31·8	3·0	0·6	2·2	37·6
Black Sea, 56 and 57	1·0	10·8	19·6	1·95	0·55	1·5	23·6
Baltic, 60	1·0	3 3	7·0	0·7	0·2	0·6	8·5
Sea of Marmora. Surface, 53	1·0	14·11	28·4	2·65	0·4	2·35	33·8
Ditto. Bottom, 52	1·0	21·0	40·4	3·55	0·9	3·2	48·05
27	1·0	21·3	42·0	3·85	0·8	2·7	49·35
Yellow Sea, 48	1 0	16·1	32·9	1·35	0·75	2·2	37·2
Mediterranean, 51	1·0	19·7	38·5	3·6	0·8	3 0	45·9
Dead Sea.	1·211	192·5	326·4	0·5	9·78	55·5	584·68
Lake Ourmia, Persia	1·16507	111·5	237·5	66·0	0·0	10 5	425·5

In these experiments the residue obtained from the water by evaporation was dried in the temperature of boiling water till it entirely ceased to lose weight. The muriate of silver was heated to incipient fusion; the sulphate of barytes and the oxalate of lime were dried at the temperature of boiling water; and the ammoniaco-phosphate of magnesia was heated to redness.

From the weights of the precipitates given in the table, it is easy to calculate the quantity of muriatic acid, sulphuric acid, lime, and magnesia, contained in the water. From these weights, together with the whole weight of salts contained in the water, it is easy to infer the weight of soda. Thus, for example, the quantity of these bodies contained in 500 grs. of the specimen of sea water marked 27 was as follows:

Muriatic acid. . . . . 8·00 grs.
Sulphuric acid . . . . . , ... 1·27
Lime. . . . . 0·314
Magnesia. 1·08
Soda . . . . 8·11

These constituents we may suppose to have been combined in the following order:

Muriate of soda . . . . 13·300
Sulphate of soda. . . . . 2·330
Muriate of lime . . . . 0·616
Muriate of magnesia . . . . 2·577
                                                    ─────────
                                                    18·823

These are the weights, supposing the salts totally free from water; but if we suppose them dried at the temperature of 212°, then the weights of each will be as follows:

Muriate of soda..................	13·3
Sulphate of soda..................	2·33
Muriate of lime..................	0·975
Muriate of magnesia..............	4·955
	21·560

Now this does not differ much from 21·3 grs. the quantity of salt stated in the table to exist in 500 grs. of the water in question.

Dr. Marcet informs us that Dr. Wollaston has ascertained by experiment that sea water contains a quantity of potash which is rather less than $\frac{1}{2000}$th of its weight. He evaporated sea water till it was reduced to about one-eighth of its volume. Muriate of platinum then threw down a precipitate, when dropped into it. When this precipitate is heated with a little sugar, the platinum is reduced to the metallic state. The muriate of potash may then be washed off, and the nature of its base demonstrated by its yielding crystals of nitrate of potash with nitric acid.

II. *An Account of the Fossil Skeleton of the Proteosaurus.* By Sir Everard Home, Bart. V.P.R.S.

III. *Reasons for giving the Name Proteo-saurus to the Fossil Skeleton which has been described.* By Sir Everard Home, Bart. —The first of these papers might have been called a short notice or explanation of three plates which accompany it, drawn by Mr. Clift and Mr. de la Beche, and exhibiting the nasal bones, the ribs, and an entire skeleton, of the fossil animal. From these plates, it is obvious that the animal had four legs, that the ribs were all bone without any cartilage in the part which connects them with the sternum, and without any joint similar to that possessed by the crocodile. The author considers the proteus, the syren of Carolina, and the axolotl of Mexico, as constituting a distinct class of animals, which he proposes to distinguish by the name of Proteus. The animal to which the fossil skeleton belonged approached the proteus in several respects, but differed in others, and appears to be intermediate between the proteus and lizzard tribe. It was to indicate these relations that our author bestowed upon it the name of Proteosaurus.

IV. *Some Observations on the Peculiarity of the Tides between Fairleigh and the North Foreland; with an Explanation of the supposed Meeting of the Tides near Dungeness.* By James Anderson, Captain of the Royal Navy.—It is the common opinion entertained by naval men, that the tide of the English channel, and the tide of the German sea, the first of which flows east, and the second south, meet at the straits of Dover, and that *this* is the reason why the tide rises to a greater height there *than* any where else in the neighbourhood. The object of Capt.

Anderson's paper is to combat this opinion, and to give what he considers as the true account of the tides in the straits of Dover, and an explanation of the phenomena. He cruized for two years in these straits, or their neighbourhood, and had in consequence an ample opportunity of making himself acquainted with the tides. The following were what he observed to take place :

Between the easternmost point of Fairleigh and the North Foreland, the tides rise from seven to eight feet higher than on either side of these points. After the tide has run east for two hours and three quarters, it is high water by the shore, and the water continues to fall during the last three hours and a quarter that the tide runs east. When the tide begins to run west, it is half ebb ; and the water continues to fall for two hours and three quarters after the tide has begun to flow west, at which time it is low water. The course of the tide continues to flow westward for two hours and three quarters longer, during which time the water gradually rises by the shore, making nearly half flood by the land at the time the current of the tide ceases to run to the westward. The tide then turns to the eastward, and after it has moved in that direction for three hours and a quarter, it is high water by the ground.

Such are the phenomena. They are obviously incompatible with the old explanation ; namely, that they are occasioned by the meeting of the two tides moving in opposite directions. Capt. Anderson ascribes them to the sudden narrowing of the English channel at Dungeness to a space not amounting to half its former breadth. This occasions the waters to rise in that narrow channel, and to continue to rise till they find a vent, which happens after the tide has flowed for three hours and a quarter from the west. By this time, all the sands without the North Foreland are covered, and afford a greater vent for the discharge of the accumulated water. The extensive flats also on both sides the channel, on which the sea now flows in like a torrent, demand a greater supply than is received through the Dungeness passage. Hence, from this period, the water is drawn off from the places where it had accumulated, and begins to fall gradually by the shore during the remaining three hours and a quarter, in which the current of the tide runs to the eastward, making half tide of ebb by the ground within the Straits of Dover and the two reservoirs, or basins, when the current of the tide ceases to run to the eastward ; at which time it is high water every where without these limits, allowing for the equalities of the coast, the water having now generally acquired a level.

When it is high water without the North Foreland, as at Margate, the Kentish Knock, &c. and the tide, which is the true or regular ebb tide, returns to the westward through the Downs, the water still continues to fall within the Foreland, and on to the easternmost point of Fairleigh, for two hours and three quarters of the first of the true or regular ebb tide ; because the

tide is falling generally, and the passage by Dungeness discharges the quantity brought by the ebb tide during that time. But when the true ebb tide has run two hours and three quarters, it is low water by the shore between the North Foreland and Fairleigh ; because the channel through the Straits of Dover (becoming again too contracted to give vent to the great body of water which now presses from the Medway and the North Sea, augmented by the currents and tides discharged from the great continental rivers and inlets) now again accumulates in the narrow passage, and in the Downs, from the North Foreland, and thus begins, from the above stated period, to rise by the shore. It thus continues to rise for the remaining two hours and three quarters, at which time the regular ebb tide has ceased to run to the westward, and it is low water every where without the North Foreland, and to the westward of Fairleigh; but within these limits, it is half flood, in consequence of the accumulation of the water during the latter part of the ebb tide.

Such is Capt. Anderson's explanation of the tides at the straits of Dover. The tide of the channel which flows *east,* continues along the French and Dutch coast till it is lost at the entrance into the Cattegat. Another portion of it goes along the Kentish coast, and gradually changing its direction, goes up the Queen's channel ; while the tide along the east coast of Great Britain, which flows south, gradually blends along with the English channel tide at the Kentish Knock, which sand has probably been accumulated by the meeting of the two tides, and proceeds up the estuary of the Thames.

Capt. Anderson shows that something very similar to the tides at the straits of Dover, happens in the narrow channel between the Isle of Wight and the English coast.

V. *On the Ova of the different Tribes of Opossum and Ornithorhynchus.* By Sir Everard Home, Bart.—In the human species, and in quadrupeds in general, the ova are formed in corpora lutea, and pass into the uterus, to the sides of which they become attached. When the fœtus is completely formed, it is expelled by the vagina, and afterwards sucks the mother. In the pullet, the yelk bags are formed in one ovarium, impregnated in one oviduct, and hatched out of the body. Our author has met with three tribes of animals which form the links necessary to connect the common quadrupeds with birds as far as generation is concerned. These are :

1. *The Kangaroo.*—In this animal the ova are formed in corpora lutea, as in the human species and common quadrupeds. They receive their yelks in the fallopian tube, and their albumen in the uterus. The ovum thus completed is impregnated in the uterus, aerated by means of lateral tubes ; and when the young has acquired the weight of about 12 grs. it is expelled from the uterus, received into the marsupium, and attached to the nipple of the mother.

2. In the American opossum, the yelk bags are formed in the ovaria, pass into the uteri, there receive the albumen, and are then impregnated. The fœtus in each uterus is aerated by one lateral tube. When expelled from these uteri, the young are received into the marsupium, and become attached to the nipples of the mother.

3. In the ornithorhynchi, the yelk bags are formed in the ovaria, received into the oviducts in which they acquire the albumen, and are impregnated afterwards. The fœtus is aerated by the vagina, and hatched in the oviduct, after which the young provides for herself, the mother not giving suck.

VI. *The Results of Observations made at the Observatory of Dublin for determining the Obliquity of the Ecliptic, and the Maximum of the Aberration of Light.* By the Rev. J. Brinkley, D.D. F.R.S. and M.R.I.A. and Andrew's Professor of Astronomy in the University of Dublin.—The mean obliquity of the ecliptic on Jan. 1, 1813, deduced from the summer solstices, was found to be 23° 27′ 50·45″. The mean obliquity of the ecliptic on Jan. 1, 1755, as deduced by Bessel from Bradley's observations, was 23° 28′ 15·49″. This gives 0·43″ for the annual diminution.

The maximum aberration of light, deduced by Dr. Brinkley from 166 observations, is 20·80″. Bessel, from Bradley's observations, has determined it at 20·70″ nearly. These are so much greater than was expected, that Dr. Brinkley thinks they will not be admitted by astronomers till verified by others. He solicits the attention of astronomers to this subject, which he considers as of great importance.

(*To be continued.*)

ARTICLE IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 20.—A paper, by W. Kitchener, M.D. was read, entitled, " On an Improvement in the Eye Tubes of portable Achromatic Telescopes." It has been long known that by increasing the distance between the two glasses next the eye and the two next the object, the magnifying power of telescopes may be nearly doubled. After several attempts to improve this principle, the author stated that he has at length succeeded, and rendered it so complete, that vision throughout a great range of power is perfect even to the edges of the field. Applied to an object glass of 30 inches focus and 2·7 inches aperture, he stated that his improved eye tube produces in the most perfect manner any intermediate power between 70 and 270, and with an achroma-

tic telescope of 44 inches focus any intermediate power between
90 and 360. The light by the use of four glasses was admitted
to be diminished, but the images of fixed stars were stated to be
better defined and rendered more distinct than by the use of any
other eye tube.

At this meeting also a paper was read, on the different Quali-
ties of the Alburnum of Spring and Winter felled Timber, by
T. A. Knight, Esq. It has been long supposed that oak timber
felled in the winter is superior to that felled in spring, but the
cause of this difference has not been inquired into, and the fell-
ing of timber in the winter has been discontinued in consequence
of the superior value of spring bark. The author proceeded
to relate some experiments he had instituted on the subject.
These were made on two similar oaks of about 100 years old,
which grew near each other, and which were felled at the differ-
ent seasons above-mentioned. The specific gravity of the
alburnum of the spring felled oak was 0·666, while the specific
gravity of that of the winter felled was 0·565. Two equal blocks
were then cut out of the alburnum of each, which, being well
dried, were suspended in a damp room for ten days: at the end
of this time it was found that 1000 grs. of the spring felled tim-
ber had gained 162 grs. while the same quantity of that felled in
winter had gained only 145 grs. Hence there was found a striking
difference between the properties of the two. Mr. Knight stated
it to be his opinion that oak timber would be much improved if
the tree, after being barked in the spring, was permitted to
stand till the following winter. He concluded by stating, that
although he had not made the experiment, he had little doubt
that the same observations would apply to the heart wood as to
the alburnum.

April 27.—At this meeting, a short abstract of a paper, by S.
Ware, Esq. on the Properties of Domes and their Abutment
Walls, was read, the mathematical nature of the subject not
admitting it to be read in detail.

At this meeting also a paper, by Assistant-Surgeon Hood,
was begun on Diarrhœa Asthenica.

May 4.—The above paper was concluded. This disease is
endemic annually among the indigent Hindus, on the Malabar
and Coromandel coast, and usually appears about the commence-
ment of the monsoons. The symptoms are diarrhœa with spasms
of the bowels and flexor muscles of the legs, sickness, &c. and
the pulse is slow and feeble. To these succeed a shivering fit,
and excessive thirst; and if proper treatment is not speedily
adopted, the pulse becomes weaker, the features contracted and
ghastly, the pain violent, and death, preceded by coma, soon
closes the scene. After discussing the various remedies which
have been employed in this disease, he proceeded to recommend
that on an attack, the patient should take two ounces of brandy
and ten drops of sulphuric acid in half a pint of cold water, and

that this dose should be repeated at proper intervals. He directed also that sinapisms should be applied to the region of the stomach and extremities to promote reaction. Bitters and astringents also were stated to be occasionally useful.

At this meeting a paper was also read, on the Mode of Formation of the Canal for containing the Spinal Marrow, and on the Form of the Fins, if they deserve that name, of the Proteosaurus, by Sir E. Home. The structure of the vertebræ of this animal was stated by Sir E. to be intermediate between that of lizards and cartilaginous fishes; and to bear so close a resemblance to those of the shark as to have been often mistaken for them. They are composed of bone, and have a body, canal for the spinal marrow, and a process for the attachment of muscles, but the body consists of one piece, while the spinous process and two lateral branches which adhere to it constitute another, and between these two pieces there is no bony union, but a species of joint peculiar to themselves. Hence the foramen in the middle thus formed appears unusually small. In the specimen from which the above description is taken there is also a fore foot, paddle, or fin (for it is difficult, according to Sir E. Home, to say which it ought to be called), which, though not quite perfect, is much more so than any that has hitherto been found. This was stated to present nothing like the thumb or claw for laying hold, which distinguishes the animals that occasionally inhabit the sea, and come to shore to lay eggs, or deposit young. If it be called a fin, it must be considered as made up of bony materials, the joints of which are very numerous, so that it may possibly perform such an office.

May 11.—A paper was read, entitled, " On the Fungi which constitutes the colouring Matter of the Red Snow discovered in Baffin's Bay," by F. Bauer, Esq. The author stated that in the winter he put a small quantity of the red globules composing the substance in question into a phial filled with compressed snow, which was placed in the open air in a north-west aspect. A thaw coming on, the snow was found melted, and the water being poured off, more snow was added. In two days the mass of fungi was observed to be raised in little pyramids, which gradually increased in height, occupying the cells of the mass of ice. A thaw now continued for some time, and the fungi fell to the bottom of the water in the phial, where they occupied a space about double that of their original bulk. These fungi also appeared to be capable of vegetating in water, but in this case they produced green, instead of red globules. By exposure to excessive cold, Mr. B. found that the original fungi were killed; but that their seeds still retained vitality, and if immersed in snow, they regenerated new fungi, generally of a red colour. The author supposes that snow is the proper soil of these fungi. The paper was accompanied by beautiful drawings illustrating the different appearances described.

<center>LINNÆAN SOCIETY.</center>

April 4.—A paper, by Mr. J. Lindley, was begun, entitled, " Observations on the natural Group of Plants called Pomaceæ."
April 18.—Mr. Lindley's paper was concluded.

At this meeting there was also read a paper, entitled, " A Systematic Arrangement and Description of the Birds of Java," by Dr. T. Horsfield.

May 2.—A paper, by Major-Gen. Hardwicke was read, entitled, " A Description of Canis Sumatrensis, viverra Lindsang, and Phasianus Cruentus."

<center>GEOLOGICAL SOCIETY.</center>

March 17.—A paper, by Charles Worthington, Esq. " On the Specimens from Devonshire," was read.

On Haldon Hill, in the road between Chudleigh and Exeter, and about five miles from the latter place, where the descent commences, the left of the road affords a good section of the materials of which this part of the hill is composed. The fragments of rock which in several places form projecting lines in the purple sand-bank, are found to possess the characters of serpentine. About a mile further, and near the lodge of Sir Laurence Palk's seat, the hill, through which the road has been cut to a considerable depth, presented the same appearances ; and here an isolated mass of porphyry was found. Without attempting to determine the manner in which these broken portions of rocks are connected with the formation of the surrounding country, it may be observed, that they approximate the granite ridge extending between Moreton Hampstead, and Bovey Tracey, and are found nearly on a parallel line to the north-west, with the greywacke formation resting on that ridge.

Among the debris of Dunscombe cliffs, about a mile eastward from the river Liu, a green hornstone, inclosed as a veinstone, may be seen passing into heliotrope and jasper.

The reading of the paper " On the Coal Fields adjacent to the Severn," by Prof. Buckland and the Rev. W. Conybeare, was concluded.

The regular coal measures succeed to the millstone grit ; more than 50 seams of coal appear to be distinguishable in this series; these are generally thin, rarely exceeding three feet in thickness, and since, from the nature of the ground, the pits often exceed 100 fathoms, and in this one instance 200 fathoms in depth, they could not be profitably worked but for the highly improved state of the machinery employed. The coal measures may be subdivided into an upper and lower series, the former being characterized by the presence of a fissile gritstone, denominated pennent, *the latter by* the predominance of argillaceous beds ; in this, *however,* some grit rocks also occur. Vegetable impressions are

abundant in many of the shale beds : nodules of ironstone occur, but in small quantity, and are not worked as ore in this field.

The coal measures are often extremely convulsed; faults which alter the level of the strata as much as 70 and 100 fathoms in some places traverse them, and on the south, where the lowest beds approach the inclined and nearly vertical calcareous strata of mendip (their fundamental rock), they become broken and contorted in a manner truly surprising; the same bed of coal being sometimes twisted into the form of the letter Z, and pierced three times in the same perpendicular shaft; near this point also a series of coal beds becomes vertical, a pit being sunk perpendicularly 80 fathoms in one individual seam; and the same series, when examined in different points along its line of bearing, is found to change the direction of its dip on the opposite sides of this vertical point in such a manner that the beds which lie uppermost in the pits on the east of that point are found (in consequence of this inverted dip) to occupy the lowest place in those on the west. Many circumstances are stated in the memoir as rendering it almost demonstrably certain that the highly inclined position of the strata, and their remarkable contortions, must be ascribed to mechanical violence which has dislocated and elevated them subsequently to their consolidation, and not to any circumstances of their original formation. These arguments are strengthened by analogies derived from the inclined strata of more recent formation in the Isle of Wight and Dorsetshire, and shown to be applicable also to the contorted strata of transition and primitive districts.

Inferences are hence drawn in favour of the elevation of many of the principal mountain chains by forces acting on them mechanically.

This part of the paper is accompanied by an appendix containing detailed lists of the strata sunk through in all the principal collieries ; many of these are very interesting, since from the depth to which the coal measures are covered through extensive portions of this field, by the more recent horizontal strata lying unconformably over them, pits have in some places been commenced even in the lower beds of the great formation of calcareous oolite, and penetrated entirely through those of lias, and of the newer red sandstone before reaching the coal. Thus the history of those formations and their relations to the coal becomes completely developed ; and since the connexions of the rocks lying beneath the coal are exhibited with great distinctness in other parts of this field, most of the obscure points in the history of the rocks occurring in the neighbourhood of this important formation will be found to receive a satisfactory elucidation from the structure of this district, especially the distinction between the formations so often confounded together of the older and newer red sandstone.

Having thus described the rocks of the coal formation, &c. as

exhibited in the basin of Somerset and South Gloucester, the memoir proceeds to notice the similar basin occupying the forest of Dean, interposing, however, some details relating to a district lying in the intermediate space between these basins, and extending from the village of Tortworth on the north of that which formed the subject of the foregoing observation towards the Severn.

This district presents, near Tortworth, two parallel bands of transition limestone underlying the old red sandstone, and distinguished by the usual characteristic organic remains.

These calcareous strata are intersected by two dykes consisting of several varieties of trap, among which an amygdaloidal species, containing grodes of brown spar, predominates ; the lime is much altered at the point where it is cut by the dyke. Thence the old red sandstone extends to the Severn at Pyrton Passage, and, crossing the river, appears on either bank near that point ; the transition limestone also makes its appearance on the southern shore.

On crossing the Severn, we enter upon the exterior chains of the forest of Dean basin. The lowest rock exhibited in these chains is a compact variety of quartzose greywacke, which forms May Hill, a low mountain half-way between Gloucester and Ross ; this is invested by mantle-shaped strata of the same transition limestone which has been noticed before at Tortworth. This commences on the south, near Hanley, in a line with the same rock at Pyrton Passage. On the north, it extends on many miles, and may be traced as far as Stoke Edith, in Herefordshire. The same strata which form the basis of the forest of Dean basin, on the north-east border, again emerge towards its opposite boundary in the centre of Monmouthshire, where a considerable tract of transition limestone ranges round the town of Usk. These two points ; viz. May Hill and Usk, correctly mark the extent of this basin. The intermediate country consists of a low mountain groupe varying from 800 to 1,300 feet above the sea. This group is traversed by the abrupt and romantic defile of the Wye. The highest strata of the basin, including the coal measures, are entirely confined to the portion of this groupe which is on the east of that river. The subjacent formations are :

1, the old red sandstone : this is here beautifully displayed, and presents beds of red sandstone and slaty marl throughout : towards its middle region occur beds abounding in calcareous matter disposed in irregular concretions, and assuming at first sight the appearance of a conglomerate : these are often worked as limestone, and usually distinguished by the name of cornstone. Near the top of the sandstone formation beds of quartzose conglomerate predominate : the total thickness of this formation is estimated at 1,460 yards. It is succeeded by No. 2, the mountain limestone, which exhibits *its* usual characters, and bears a thickness of 165 yards. It

here contains beds of iron ore associated with brown spar, &c.
These furnish the iron ore worked in the forest, the coal mea-
sures being almost destitute of that mineral. The coal formation
which occupies the highest place in this basin includes in all 22
beds of coal alternating with slate clay and various sandstones,
the total thickness of the formation being about 800 yards. In
this part of the memoir, the authors profess to derive the most
important parts of their information from Mr. Mushet's section.

The horizontal and overlying strata are next described. These
consist of 1, extensive beds of a conglomerate, containing rolled
and angular fragments derived from the partial destruction of the
older neighbouring rocks, and, therefore, containing in this dis-
trict pebbles and boulders of mountain limestone as the principal
ingredient, associated with others of old red sandstone and
quartz. These beds have evidently been originally accumulated
round the bases of the older chain in the form of coarse gravel,
and afford a proof of the convulsions, they must have undergone
antecedently to the deposition of the more recent strata. The
cement of these conglomerates is almost always calcareous, and
often consists of magnesian carbonate of lime. From the gradual
disappearance of the pebbles in portions of these beds, they
frequently pass into an homogeneous magnesian limestone
entirely agreeing in character, as well as geological position,
with the magnesian limestone in the north-east counties of
England. These conglomerates are often metalliferous, contain-
ing galena and calamine, and form the country of some of the
principal mines of Mendip. These beds graduate into those of
No. 2; they are considered as subordinate to it, and both are
included under the same general formation.

No. 2, beds of tender red and variegated sandstone and marl;
these are considered as composing, together with the preceding,
the newer redstone formation, the conglomerates occurring in the
lower, the sandstones in the middle, and the marls in the upper
part of the series. The sandstones and marls contain gypsum
and sulphate of strontian in nodules and veins.

Upon this newer sandstone reposes the well-known formation
of lias. The compact earthy limestones so denominated occupy
a thickness of about 50 feet, being covered by about 150 feet of
blue marl, alternating with a few thin seams of marly limestone.
The white lias, which affords slabs proper for the purposes of
lithography, occupies the lowest place in the series. Above the
lias commences the series of oolites, No. 4, the members of which
occur in the following order: 1, sandstone, with calcareous
concretions; 2, the inferior oolite, a coarse oolitic freestone;
3, clay and fuller's-earth; 4, the great oolite, which crowns the
whole escarpment and elevated platform of the Cotswold hills,
and their prolongation in Somersetshire and Wiltshire.

The edge of this escarpment is furrowed by deep valleys,
frequently cutting through and exposing in section all the strata

from the great oolite to the newer sandstone; and the whole of
the district occupied by lias is traversed and intersected by simi-
lar vallies in such a manner as to divide it into numerous insu-
lated platforms based upon the sandstone, and often crowned
with lofty summits of oolite, these summits being frequently
many miles from the main chain of the oolite formations. These
insulated groups minutely correspond with each other, the same
strata being always found on the opposite sides of the valleys
dividing them in the continuation of the same planes.

These circumstances are strongly insisted on as affording a
proof almost demonstrative that the valleys in question owe
their origin to some cause which has excavated them subse-
quently to the deposition and consolidation of the strata which
have been thus removed by intervals once occupied by them.

Alluvial detritics is less abundant than might have been
expected in such a country. The flats in which it might have
been looked for being generally covered with marshes; deposits
of it, however, are scattered in several places, which have
afforded the usual remains of elephants, &c. The most remark-
able accumulation of this kind occurs under very peculiar
circumstances, occupying a fissure and cavern in the mountain
limestone of Hutton Hill, one of the Mendip chain. This cavern
is nearly filled with an ochreous deposit so pure as to have been
an object of extraction, mixed with boulderstone and pebbles :
dispersed through this mass were found great abundance of
bones of land animals : magnificent molar teeth of the elephant,
and an entire skeleton of an animal of the dog tribe, probably a
fox. There can be very little doubt that the famous skeleton of
the rhinoceros discovered at Plymouth really occurred under
similar circumstances, although the workmen neglected to
notice the exterior opening of the cavity in which it was buried,
which might, perhaps, have been closed by stalactites, a sub-
stance easily confounded by such observers with the native
rock.

The caverns of Hutton Hill are no longer open, but they are
minutely described in the MSS. of the Rev. Mr. Calcott, pre-
served in the city library of Bristol; and numerous specimens of
the fossils are deposited in his cabinet in the same building.

ROYAL ACADEMY OF SCIENCES AT PARIS.

*An Analysis of the Labours of the Royal Academy of Sciences
of Paris during the Year* 1818.

(*Continued from* p. 388.)

BOTANY.

THE first known and the most useful of the palms is undoubt-
edly the date tree; it is one of the most valuable productions of

Barbary and Egypt, and is also cultivated with advantage in several of the southern countries of Europe. M. Delisle, who carefully observed the cultivation of it, while he was attached to the expedition to Egypt, described it very fully in a memoir which he presented to the Academy. This tree is cultivated from seeds, from suckers, and even from slips. The mode of treatment of the slip, which consists in replanting the top after having separated it from its trunk, had been already mentioned by Theophrastus and by Pliny; and M. Delisle was assured by the Arabs that it is still practised. It is well known that the date tree has the sexes separately on different plants; the suckers of each tree producing plants of the same sex. The inhabitants, in order to gain as much profit as possible from their land, take care to plant no more than the small number of males which are requisite for the artificial fecundation of the females, and if, from any cause, the catkins of these male date trees should not be placed at a proper time in a situation to throw their fertilizing farina on the female flowers, the fruit will not ripen, and the crop is lost.

A species of palm much less known than the date is that of the nipa, which grows spontaneously in the Indian Archipelago, on the sea coast. Rumphius and Thunberg have given imperfect descriptions of it; the young kernels of it are eaten when preserved. Its catkins cut before it is fully expanded produces a sweet liquor, which, by fermentation, become spirituous and pleasant to drink. Baskets, mats, and other trifling articles, are made of the leaves.

M. Houton Labillardiere observed, and carefully describes, the fructification of it; and has in several instances rectified the opinions hitherto entertained of it. The female flower has three stigmas, and the young fruit three ovales; the embryo is placed at the foot of the seed. In respect to its male catkins, with sessile flowers, its antheræ borne on a single filament, which is not ramified; its female flowers without a calyx, and its aggregate fruits, it strikingly resembles the pandanus; but its spathæ, the calyx of the male flowers in six divisions, and the fan-like form of the leaves, produce a still nearer degree of affinity to the true palm trees.

The ancients make frequent mention of an Egyptian tree to which they give the name of *Persea*: it resembled a pear tree, but its leaves lasted during the whole year; its stone fruit was very sweet and wholesome, and the wood, which was black and hard, was extremely valuable. In the Arabian writers of the middle ages, we may still find descriptions of a tree which they call *leback*, and which offers all the characters attributed by the ancients to their *persea*, but this tree has latterly become so rare, at least in Lower Egypt, that botanists have not been able to fix upon it with certainty; some of them, as Clusius, and Linnæus.

upon his authority, have given the name of persea to a species of laurel, an opinion which is the less admissible as this laurel comes from America. Others, as Schreber, have fancied they found it in the sebestier (cordia mixa) whose viscous fruit is, however, quite different. M. Delisle was more fortunate, having observed in a garden in Cairo a specimen of the tree called by Linnæus *ximenia ægyptiaca*, he perceived it possessed most of the characters of the persea, the height was from 18 to 20 feet, the branches thorny, and the oval perennial leaves more from one inch to an inch and a half in length, which traits may have occasioned its comparison with the pear tree ; its fruit is in the form of the date ; is sweet when ripe, and contains a kernel which is rather ligneous. When M. Delisle arrived in Upper Egypt, he met with two others, and he learned from the inhabitants of the higher country, that it is common in Nubia and in Abyssinia, and much esteemed in Dafour. Nevertheless he could not learn whether the inner part of the wood is black, as the ancients say is the case with respect to their persea.

The tree is now called in Nubia *eglig*. M. Delisle remarked in it peculiarities sufficiently marked to separate it from the other ximenia, and he made it a genus, to which he gave the name of *balanites*.

Among the vegetables, which furnish a juice of a milky appearance, one of the most remarkable is that which the Spanish colonists have called the cow tree, because its milk, far from having, like that of the spurges and most other lactescent plants, acrid and pernicious qualities, yields, on the contrary, a wholesome and agreeable beverage. M. de Humboldt read to the Academy a description of this tree, and of the experiments made upon the juice which it supplies. This celebrated traveller, not having been able to see it in flower, has not settled its genus; but to judge from its fruit, it seems to belong to the family of the sapotillæ ; it is tall ; its leaves are eight or ten inches long, alternate, coriaceous, oblong, pointed, and marked with lateral and parallel ribs.

When incisions are made in it, a glutinous milk runs out, with a very pleasant balsamic smell, of which the negroes drink large quantities, dipping into it maize bread, or tapioca, and this food sensibly fattens them. When exposed to the air, some pellicles are formed on the surface which acquire as they dry somewhat of the elasticity of the caoutchouc, and a curd is separated, which becomes sour in time, and to which the common people give the name of cheese.

M. de Humboldt takes this opportunity of making some general reflexions on the different vegetable milks, whose injurious qualities depend on certain poisonous principles, which exist in a sufficiently large quantity to produce sensible effects, such as *the morphium* in opium, but in the most poisonous families,

there are some species of which the juice is not poisonous, as the *euphorbia balsamifera* of the Canaries, and the *asclepias lactifera* of Ceylon.

Messrs. de Humboldt and Bonpland are continuing the publication of their great botanical work; it is entitled " Nova genera et Species plantarum Equinoctialium."* The third volume, which will be finished in a few months, and the fourth, which is already printed, but not yet published, will complete the series of monopetalous plants. These four volumes contain more than 3000 new species, divided into 623 genera, of which nearly 100 are new. M. Kunth, who is a correspondent of the Academy, and has undertaken the publication of this work, describes in the family of the composite plants nearly 600 species classed according to a method peculiar to himself. Some notes, which M. Humboldt has added, give an account of the heights at which the plants of the cordilleras grow, and contain observations on the distribution of vegetables upon the surface of the earth.

Two volumes still remain to be published; they will be devoted to the polypetalous plants.

But as the plan adopted respecting the *nova genera et species* does not admit of giving figures of all the plants collected by these travellers, M. Kunth has begun to give, in a separate work, entitled, " Mimoses et autres Plantes du nouveau Continent de la Famille des Legumineuses," a selection of the most beautiful species. The plates, which are executed with all the splendour which French engraving has attained, will be accompanied by a general work on leguminous plants. The figures belonging to the first number of this monography have been presented to the Academy.

In order to assign to each genus its place in the natural order, M. Kunth was obliged to study particularly all the families of the plants, to examine the immense number of genera and species preserved in herbariums, and to consult all the authors who had already treated of the same subjects. In consequence of these researches, he has given in separate memoirs general remarks on the families of the gramineæ, of the cyperaceæ, of the piperaceæ, of the aroideæ; and since that he has considered the family of the bignonaceæ. The object of these memoirs is either to point out the groupes or subdivisions which may be established in these families, or to fix the characters of their genera with more precision.

At the same time, Mr. Hooker, the learned author of the Monograph of the Jungermannia, is continuing to publish in London the cryptogamous plants which M. Humboldt entrusted to his care. He has united these plants to those collected by

* Nova genera et species plantarum quas in peregrinatione ad plagam æquinoctialem orbis novi collegerunt, descripserunt et adumbraverunt Am. Bonpland et Al. de Humboldt, Ex schedis autographis A. Bonplandii in ordinem digessit C. S. Kunth.

M. Menzies. M. Hooker's work bears the title of " Musci Exotici."

M. Beauvois continues with the same perseverance the publication of the plants he collected in his travels; and this year there has appeared the 17th number of his " Flore d'Oware et de Benin," which we have already mentioned several times.

ZOOLOGY.

The Count de Lacepede, having had the use of some very highly finished paintings, brought from Japan by the late M. Titsing, representing many subjects of natural history, of which those which were known to us are given with great exactness, thought he might regard these paintings as documents sufficiently authentic to establish even the species which are not known in any other way. In consequence of this, he composed from them a description of several cetaceous species which have not yet been observed by European naturalists. These consist of two whales, properly so called; that is to say, without a dorsal fin, four balænoti, or whales, provided with such a fin, one physeter, or cachelot, with a dorsal fin, and one dolphin.

The author gives a detailed account of the distinctive characters of these eight animals, forming a considerable addition to the catalogue of known cetaceous animals, which, in the last work of M. de Lacepede on this class, did not exceed 34.

M. Cuvier has presented the head of an orang-outang, of middle age, sent from Calcutta by M. Wallich, Director of the Garden of the Hon. East India Company. He remarks that the heads of orang-outangs hitherto described were all taken from very young subjects, which had not yet changed their first teeth, that which he placed before the Academy, being of maturer age, has a more prominent snout, and more receding forehead; some traces of temporal or occipital crests, may be perceived in it, which occasion a resemblance to the head of the large monkey, known by the name of the pongo of Wurmb. The latter having, besides all the sutures, forms, proportions, foramina, and cavities, characteristic of the orang-outang, it is not impossible that the large monkey of Wurmb may be nothing more than a common full-grown orang-outang. At all events it is certainly a species of orang, although M. Cuvier himself has, from the comparative smallness of the scull, been led into the error of placing it among the mandrills, and other long snouted monkeys. The same member has exhibited the figure of a tapir, a native of Sumatra, and now alive in the menagerie of the Governor-General of the English East Indies, the Marquis of Hastings, it differs from the American tapir, in respect to the whitish colour of part of its back, while the rest of its body is of a very dark brown. It appears from a memoir which accompanied this drawing, and which was sent to M. Cuvier, by M.

Diard, a young naturalist in the East Indies, who is occupied in scientific researches, that this species of quadruped inhabits not only the island of Sumatra, but likewise a part of India beyond the Ganges. Hitherto the genus of the tapirs had been supposed peculiar to America.

M. Moreau de Jonneo, a correspondent of the Academy, who intends to describe particularly the different reptiles of the Antilles, and who began that work last year by a very detailed history of the famous yellow viper, or fer de lance of Martinique, presented this year to the Academy a memoir on the species of gecko, called in that island mabouia des murailles, and which is no other than the thorny-tailed gecko of Daudin. This animal, which has a hideous aspect, and talons which give him the faculty of fastening himself so as to walk along ceilings, inhabits the interior of houses, where it principally pursues the cockroaches.

The inhabitants have a great dread of it, attributing noxious qualities to it, and have given him the name of mabouia, because it is that by which the evil spirit is known among the Caribbees. It is the same animal, of which Acrelius relates, that it spits out a black and venomous saliva, and it has been mentioned, but very ill described, by several naturalists under the name of spectator. There is another species of gecko called in the Antilles, mabouia of the Bananas; it grows to a larger size, and is the smooth gecko of Daudin; and its tail, after having been pulled off, frequently grows again larger than it was at first.*

These remarks are the more interesting, as some naturalists had erroneously given the name of mabouia to a species of scinque.

The same writer has given another memoir respecting a species of coluber, which, from its agility, has acquired the name of runner (coluber cursor gm). It is a timid innocent animal, destroying a great number of snails, and very carefully protected by the inhabitants, because they suppose it to be the bitter enemy of the fer de lance viper, but that is an error, owing, according to M. de Jonnes, to their having confounded it with a large species of boa, which no longer exists at Martinique.

The large works on zoology published by the academicians, have been continued with zeal. There has appeared one volume of the Animals without Vertebræ, by M. Delamarck, and also some numbers of the "Zoological Observations" of M. Humboldt, and of the Insects of Africa, by M. de Beauvois.

ANATOMY AND PHYSIOLOGY.

We have already, in our analysis of last year, given a very

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* The gecko with a thorny tail, the gecko porphyré, and the spectator, are, according to M. Moreau de Jonnes, the same animal; they belong to the family of the hemidactyl geckos. The smooth gecko, and the gecko with a swollen tail, are also the same, and belong to the thecadactyls.

detailed account of the important researches, by means of which
the Chevalier Geoffroy Saint Hilaire has endeavoured to form a
comparison between the bony parts of the branchial apparatus
in fish, and those which perform analogous functions in the
skeleton of the three other classes of animals with vertebra.
This learned naturalist has this year presented to the Academy
several new memoirs on the same subject, and he has published
the whole in one volume, under the title of " Anatomical Philo-
sophy, or on the Respiratory Organs, considered with Respect to
the Determination and the Identity of their bony Parts," with
10 mezzotinto plates.

The work of M. Geoffroy may be considered in three different
points of view; it embraces,

1. The enumeration and description of all the bones com-
posing each of the organs which contribute to respiration in fish,
and of those of some of the other classes, whenever it was
necessary to the plan of the author to describe them anew.

2. The resemblances admitted by the author between the
bones, which had hitherto been supposed to belong exclusively
to fish, and those which he considers as analogous to them in
other animals with vertebræ.

3. The conclusions which he is led to form from these newly
discovered resemblances, as far as regards the nature and the
destination of the organs to which these parts belong.

M. Geoffroy has thus carefully enumerated and described all
the minute parts which enter into the large branchial apparatus;
those which form the boney arches on which the gills are sus-
pended ; those which support those arches ; those annexed to
them called the pharyngial bones ; those which cover them, and
bear the name of operculum, &c.   He informs us of how many
bones the sternum is composed in the different classes of
animals with vertebræ, and how these parts are arranged in
them.   He also gives new and curious details respecting the
composition of the different os hyoides, ánd respecting the points
of ossification, which are to be met with in the cartilages of the
different larynges, and likewise on the resemblance between the
upper larynx of birds and that of the mammalia.

This part of his work, which consists of positive facts, most of
them new, and all clearly described, will always remain a valuable
acquisition to science.

The second part, which establishes the analogy of the bones
of which we have just spoken with those of the superior classes,
presents much greater difficulties, as may have been seen in our
last analysis.

According to M. Geoffroy, the bones which form the gill
covers correspond with the frame of the tympanum and the small
bones of the ear, the bones which bear the branchiostegal mem-
brane, proceed from an intermixture or intercalation of the pieces
of the sternum, between those of the os hyoides; from a change of

position of this bone, whose thyroidean processes, which in the mammalia are directed backwards, and united with the thyroid cartilage, are here carried forward, and changed into a lingual bone ; and lastly, from a removal of the sternum, from the place it occupied in the first three classes behind the clavicles, or the os coracoides, to the front of these bones where it is placed under the throat. The lateral pieces which unite the arches of the gills to the ligament which carries them, correspond, according to M. Geoffroy, with the points of ossification of the thyroid cartilage, and with the arythenoid cartilages ; the lower pharyngean bones with those of the cricoid cartilage ; the upper ones to a flat piece which may have detached itself from the sphenoid bone, or with the cartilaginous part of the eustachian tube ; the branchial arches with those of the bronchiæ, and the small bones which stand out from them to the rings of the trachea. We have already given an account of these resemblances in our preceding analysis, and we can now do no more than refer to the full explanation which M. Geoffroy gives of them ; all the reasons which induced him to give to each the degree of probability it is susceptible of are there detailed.

With respect to the third order of M. Geoffroy's ideas, those concerning the really essential functions of the organs, it may be asserted that they arise partly from the above-mentioned researches, and have partly been formed in order to add weight to the results he draws from these researches.

Consequently M. Geoffroy, being firmly convinced that the perfectly developed bones which compose the gill covers of fish, and which in that class do not appear to serve for hearing, are nothing else than the malleus, the incus, and the other small bones of the ear of the mammalia on a larger scale, must have been led to doubt whether these bones are the organs of hearing, even in the animals in which they have always been considered as such, and to regard them merely as a sort of superfluity which has remained incipient (these are his own terms) in the animals with lungs, and indicative of an organization which is vigorously necessary and amply developed in fishes.

In the same way, having supposed that he found in the bony apparatus of the gills, which produce no sound, all the bones of the larynx, he has been induced to believe that " it is not upon true and solid foundations that the larynx has been represented as destined for the voice, as the principal organ of the voice," and he prefers calling it " the first ring of the windpipe, the station for the governors of the breathing organ, and the assemblage of its most zealous attendants."

Nevertheless it is our duty to observe, that on the latter subject M. Geoffroy is not so hostile to the received opinion as the efforts he makes to support his own might induce one to believe; for he does not dispute that in animals with lungs the larynx serves for the voice, and he even establishes a new theory to *explain how that organ* performs this function. This is also the

case with respect to that part of his work in which M. Geoffroy
combats the existence of a lower larynx in birds ; not that he
denies that they have at the bottom of their tracheæ organic dis-
positions which produce sounds ; he only means to say that
these dispositions do not consist in parts similar to those of the
upper larynx, which no person indeed had ever ventured to
assert.

The theory peculiar to M. Geoffroy of the voice and of sound
is not necessarily dependent on his anatomical researches ; it is
the result of some ideas of general natural philosophy, which he
long since formed, but which he has not, on the present occa-
sion, developed sufficiently to allow of our giving an account of
it.   We shall merely say, he considers the thyroid cartilage as a
sonorous body, which serves as a table of harmony to the vocal
instrument, and attributes to the variation of the distance between
this cartilage and the os hyoides the variations of tone.

This volume is terminated by a memoir on the bones of the
shoulder.   The author long ago made known the resemblances
of these bones in fishes to analogous bones in birds, and he has
indeed been led by this circumstance to make all those researches
on comparative osteology, of which we have more than once
spoken to our readers.   He has resumed this subject in a more
general point of view, and considers these bones as having
attained in fish their maximum of development and importance,
by serving as a shield to the heart, a support to the diaphragm,
and a stretcher to the gills.

To conclude, we shall here repeat the invitation we have
already given to naturalists, to consult a work filled with new
and interesting facts, and from which much may be learned
even on those points, respecting which we may not be able to
adopt all the opinions of the author.

M. Edwards has continued the curious experiments which he
began last year on the respiration of frogs ; he had then con-
vinced himself the presence of air is useful in prolonging the
life of these animals, when circulation and pulmonary respiration
have ceased ; that water causes them to perish more quickly than
a solid covering, and the more quickly in proportion as it is less
impregnated with air ; and he has this year occupied himself
more particularly about the influence of the air contained in
water, and that of the temperature to which this liquid is raised.
He has proved that the deleterious effects of the water diminish
with the temperature.   Frogs have lived twice as long in water
at 10° (50° Fahr.) as in water at 15° (59° Fahr.), and thrice as
long in water at 0° (32° Fahr.).   On the contrary, their life is
shortened by nearly one half at 22° (72° Fahr.) by more than
three quarters at 32° (90° Fahr.) and they perish instantaneously
upon being plunged into water at 42° (107° Fahr.)   The coldness
of the atmosphere before the operation is also a circumstance
favourable to the lengthening of their existence in cold water.
The quantity of air contained in the water, the volume of the

water employed, and the frequent renewal of that water, are circumstances which likewise contribute to it, each in such proportions, and with such limitations, as M. Edwards has determined by numerous experiments, which were made with every precaution of the most accurate natural philosopher.

Frogs can live for several months between 0° (32° Fahr.) and 10° (50° Fahr.) in a quantity of 10 quarts of aerated water, which is renewed once every day; the action which the air of this water exercises on their skin is sufficient for their existence without their lungs being required to be brought into play, but at 10° and upwards they can only live by rising to breathe the air on the surface. If they are confined under the water at 12° or 14° (53° or 57° Fahr.) for example, they perish, whatever care may be taken to renew it, in one or two days; a running stream will sometimes enable them to support a more elevated temperature under water; some of them support it as high as 22° (or 72° Fahr.)

These experiments, independently of their interest with respect to the general theory of the action of air on the blood, explain several singular traits in the economy of these animals, and particularly the extraordinary difference in their manner of living in winter and in summer.

(To be continued.)

Article X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Population of Glasgow.*

An actual survey to determine the population of Glasgow was terminated on Feb. 26, 1820. The following is an abstract of the information derived from this survey:

Population of the 10 parishes within the Royalty 75,169

Barony Parish.

Anderston district 7,113
St. Vincent-street and the Blythswood
    estate district 7,941
Port Dundas district 7,598
Calton and Mile-end district 15,616
Bridgeton district. 13,593

                             51,861

Gorbal's parish, including Hutchesontown, Lauriestown,
    and Tradestown. 21,768

                             148,798

As several thousand persons had left the population district for want of work during the few months which preceded the enumeration, and as some of these persons may be expected to return, the population may be safely stated at 150,000.

II. *Colouring Matter of a Lichen which grows on the Bark of the Brucea Anti-dysenterica.*

This lichen had an intense yellow colour. It was subjected to a chemical examination by MM. Pelletier and Caventou. They first digested it in sulphuric ether till every thing soluble in that principle was taken up. Nothing was dissolved by the ether but a solid oil, of a greenish-yellow colour, and a mild taste, and possessing the usual characters of the fatty matter of plants. The lichen was now digested in successive portions of hot alcohol, till that liquid refused to dissolve any thing more. The alcoholic solutions had a very intense colour. The alcohol was distilled off, and there remained a yellowish-red matter, which attracted humidity when exposed to the air. Water being poured upon it dissolved a yellow-colouring matter, and left a brick-red powder, which was destitute of taste and smell, and perfectly insoluble in water. By boiling it repeatedly in water, it was freed from the remains of the soluble colouring matter, which still might remain mixed with it.

This substance is a powder of a reddish-yellow colour, insoluble in cold water, and almost equally so in boiling water. It dissolves with facility in alcohol; but is insoluble in sulphuric ether. When exposed to heat, it gives out the usual products of vegetable bodies with some traces of ammonia, indicating the presence of azote as one of its constituents. When concentrated nitric acid comes in contact with it, it assumes a fine green colour. This colour is removed by the action of water, and by most of the salifiable bases. It is owing to a combination of the colouring matter of the lichen with nitric acid.

Sulphuric and muriatic acid facilitate the solution of this colouring matter in water; but they at the same time alter its nature; for it is now very soluble, and is not rendered green by the action of nitric acid. This colouring matter is not acted upon by weak alkaline leys; but the concentrated solutions of the alkalies decompose it.—(Journ. de Pharm. v. 546.)

III. *Curious Effect produced by kneading powdered Guaiacum and good Wheat Flour.*

M. Taddey observed that when the powder of guaiacum and good wheat flour are kneaded together with the requisite quantity of water in contact with atmospherical air, the mixture assumed a fine blue colour. M. Rudolphi, whom he employed to examine the action of these bodies on each other, made the following observations: 1, when the powder of guaiacum and pure stock are kneaded together, no blue colour is developed; 2, the powder of guaiacum scarcely becomes blue when kneaded with

any meal or flour, which contains but little gluten ; 3, it does not acquire a blue colour, when the wheat flour has undergone any great alteration in its qualities ; 4, when gluten or zimome is kneaded with powdered guaiacum, a very fine blue colour is instantly developed.

From these observations, Rudolphi concludes that the powder of guaiacum is an excellent reagent for determining whether wheat flour be of a good quality, and whether it has undergone any alteration.—(Giornale di Fisica, Chemica, &c. second bimestre, 1819.)

IV. *Substances capable of developing a Blue Colour in the Alcoholic Solution of Guaiacum.*

From the experiments of M. Planche, it appears that the fresh roots of the following plants are capable of producing a blue colour, when introduced into an alcoholic solution of guaiacum:

Symphytum consolida,	Arctium lappa,
Leontodon taraxacum,	Colchicum autumnale,
Iris germanica,	Saponaria officinalis,
Cichorium intybus,	Fumaria officinalis,
Eryngium campestre,	Cochlearea officinalis,
Nymphæa alba,	Scrophularia officinalis,
Solanum tuberosum,	Rumex acetosa,
Bryonia dioica,	Scorzonera hispanica,
Inula helenium,	Asparagus officinalis,
Althæa officinalis,	Borago officinalis,
Daucus carota,	Angelica archangelica,
Glycyrrhiza glabra,	Allium cæpe.
Napis sativa,	

V. *Substances which do not strike a Blue Colour with the Alcoholic Solution of Guaiacum.*

M. Planche has found that the fresh roots of the following plants do not affect the colour of the alcoholic solution of guaiacum :

> Rumex acutus,
> Polypodium filex mas,
> Fragaria vesca.—(Journ. de Pharm. vi. 18.)

VI. *Indigo.*

The indigo of commerce is very far from pure, being mixed with nearly half its weight of foreign substances. I attempted to obtain it in a state of purity by sublimation ; but after a good many trials, I was obliged to renounce that method without accomplishing my object ; for how carefully soever I regulated the heat, I always found that at the subliming temperature, the greatest part of the indigo was destroyed. I succeeded, however, in procuring a few grains of pure indigo by sublimation, which I sub-

jected to analysis, by heating it to redness with peroxide of copper; but my stock of indigo was exhausted before I was able to satisfy myself with regard to the proportions of the different constituents. I, therefore, had recourse to the indigo vat, as it is used by the calico printers, and by means of it easily procured as much pure indigo as I had occasion for.

In the indigo vat, as used by the calico printers and dyers in general, the indigo is deprived of its blue colour by means of protosulphate of iron, and then dissolved in water either by means of an alkali or of lime. The solution is greenish-yellow, and when lime is the solvent, the quantity in solution did not appear in various trials which I made ever to exceed what lime-water was capable of dissolving. It was only necessary to dip a glass phial pretty deep in the indigo vat, and fill it with the clear solution. On pouring the liquid from the phial into another vessel in the open air, the indigo immediately absorbs oxygen, recovers its blue colour, and becomes insoluble in water. By digesting the blue pigment thus obtained in dilute muriatic acid, I removed all the carbonate of lime with which it might be mixed, and even the iron, if any happened to be present. The residual blue powder I considered as pure indigo. By repeated trials with peroxide of copper, I satisfied myself that the constituents of indigo are as follows :

$$
\begin{aligned}
7 \text{ atoms carbon.} &\dots\dots\dots\dots\dots\dots = 5\text{·}25 \\
6 \text{ atoms oxygen.} &\dots\dots\dots\dots\dots\dots = 6\text{·}00 \\
1 \text{ atom azote} &\dots\dots\dots\dots\dots\dots\dots = 1\text{·}75 \\
\hline
& 13\text{·}00
\end{aligned}
$$

So that it contains three different constituents, and is a compound of 14 atoms. The weight of an integrant particle of it is 13.

It appears from this analysis that indigo contains a very considerable proportion of oxygen; for its constituents in the 100 parts are as follows :

$$
\begin{aligned}
\text{Oxygen} &\dots\dots\dots\dots\dots\dots\dots\dots 46\text{·}154 \\
\text{Carbon.} &\dots\dots\dots\dots\dots\dots\dots\dots 40\text{·}384 \\
\text{Azote} &\dots\dots\dots\dots\dots\dots\dots\dots 13\text{·}462 \\
\hline
& 100\text{·}000
\end{aligned}
$$

Indigo, when it becomes soluble in alkalies or alkaline earths, always loses its blue colour, and becomes greenish-yellow. The instant that this solution is exposed to the air, or to oxygen gas, the indigo recovers its blue colour, and falls down in an insoluble powder. Hence it is obvious that it acquires its blue colour by absorbing oxygen, and consequently that the blue pigment contains more oxygen than the greenish-yellow. I ascertained by trial how much indigo was contained in a given weight of the

8

greenish-yellow solution from the indigo vat. I then let up a determinate quantity of the liquid into a graduated glass tube filled with mercury, and standing on the mercurial trough. This done, I let up a certain number of cubic inches of oxygen gas into the same tube, and allowed the tube to remain inverted over the mercury till the whole of the indigo was precipitated in the state of a blue pigment, and till the oxygen gas ceased to diminish in bulk. The loss of bulk which the oxygen gas sustained, together with the known weight of the indigo present, enabled me to determine how much oxygen was necessary to convert the greenish-yellow soluble pigment into blue insoluble indigo. The result of three experiments made in the way just described was nearly the same, and was as follows:

Indigo in the state of a greenish-yellow soluble pigment, or the soluble basis of indigo, as it is called, is composed of

$$
\begin{aligned}
5 \text{ atoms oxygen.} &\ldots\ldots\ldots\ldots\ldots = 5{\cdot}00 \\
7 \text{ atoms carbon.} &\ldots\ldots\ldots\ldots\ldots = 5{\cdot}25 \\
1 \text{ atom azote.} &\ldots\ldots\ldots\ldots\ldots = 1{\cdot}75 \\
\hline
& 12{\cdot}00
\end{aligned}
$$

So that it is a compound of 13 atoms, and the weight of an integrant particle of it is 12. The addition of a single atom of oxygen renders the colour blue and the pigment insoluble. Thus it appears that the blue pigment differs from the greenish-yellow soluble basis merely by containing one additional atom of oxygen.

Thus indigo exhibits a striking refutation of the old notion that acidity is owing to the union of oxygen with an acidifiable basis. The blue pigment is soluble in sulphuric acid, and when recently obtained by precipitation, it may be dissolved in several other acids ; but no alkaline substance that I have tried is capable of combining with it. Hence it appears to possess alkaline properties, or at least to approach much nearer the nature of a salifiable base than of an acid ; but when we deprive it of an atom of oxygen by means of protosulphate of iron, or any substance which has a strong affinity for oxygen, it acquires a greenish-yellow colour, and becomes capable of combining with the alkalies, and with lime, barytes, and strontian, and perhaps also with other salifiable bases. It has, therefore, acquired acid properties, or at least approaches much more nearly to the nature of an acid than it did while in the state of a blue pigment. Thus the addition of oxygen gives indigo alkaline qualities, and the abstraction of oxygen gives it acid properties.

Should any person think of repeating these experiments, it may be necessary to put him on his guard against a resinous substance, which indigo often, if not always, contains, and which I have found to dissolve with it in alkalies and lime-water, and, therefore, to contaminate the pure indigo obtained from the indigo vat. *Its* presence for a long time deceived me, and led

me into the notion that hydrogen was a constituent of indigo. It is obvious that this resinous substance is easily got rid of by digesting the indigo recovered from the indigo vat in a sufficient quantity of alcohol.

VII. *Common Rosin.*

Modern chemists have very much neglected the characters of resinous bodies, though the science is now sufficiently advanced to enable us to discriminate them with precision from each other, and even to determine their constituents. I intend, therefore, occasionally to introduce into the *Annals of Philosophy* an account of the experiments which I have occasionally, and at long intervals, made upon these bodies, both with respect to their characters and composition. I shall begin with common rosin, as constituting the common type to which all the others have been referred.

Common rosin, as purchased in the shops, is a semitransparent brittle resin, of a yellow colour. Sometimes it has a slight smell of turpentine, and sometimes is nearly destitute of all smell. In the first case, it is probably contaminated by some oil of turpentine.

The specific gravity of common rosin I found to be 1·080. Alcohol, of the specific gravity 0·835, dissolves the eighth part of its weight of it at the temperature of 60°. The solution is yellowish, and perfectly transparent. When exposed to a moderate heat, the alcohol flies off, and leaves the rosin in the state of a semitransparent mass, of a much darker-brown colour than before the solution, and exactly similar in appearance to rosin that has been kept for some time in a state of fusion.

When common rosin is heated to the temperature of 156°, it becomes viscid, and of the consistence of common turpentine. As the heat increases, the rosin swells up, and becomes filled with bubbles. This is owing to a quantity of water, and probably also of oil, which is separated from it by the action of the heat. When heated to the temperature of 276°, it becomes quite fluid; and if it be kept a sufficient time at that temperature, it loses all its water, and remains in the state of a reddish-yellow liquid. When allowed to cool, it concretes into a reddish-yellow rosin; much darker coloured than before, and obviously altered in its constitution.

To determine the constituents of rosin in the state in which we find it exposed to sale, I heated a grain of it in the apparatus, which I have described in a former paper, with peroxide of copper. The mean of two experiments, which scarcely differed from each other, gave the following results :

5 cubic inches of carbonic acid .. = 0·6324 gr. carbon
1·05 gr. of water .... = 0·1164 gr. hydr.

Total . ... = 0·7488
Deficiency. .... = 0·2512 gr.

This I ascribe to oxygen which the rosin contained. Hence it follows that a grain of rosin is composed of the following constituents :

                    Carbon. .... 0·6324
                    Hydrogen. .... 0·1164
                    Oxygen .... 0·2512
                                                    ‾‾‾‾‾‾
                                                    1·0000

Now if we convert these respective weights into volumes, we shall find them as follows :

                    Carbon. 5   cubic inches
                    Hydrogen. 5½
                    Oxygen. .... 0·75

Now a volume of carbon and hydrogen is equivalent to an atom, and half a volume of oxygen is equivalent to an atom. Of consequence the constituents of common rosin in atoms are as follows :

            5   atoms carbon .... = 3·7500
            5½ atoms hydrogen .... = 0·6875
            1½ atom oxygen .... = 1·5000
                                            ‾‾‾‾‾‾
                                            5·9375

Or, doubling each constituent to get rid of the half atoms, we obtain

            10 atoms carbon. .... = 7·500
            11 atoms hydrogen. .... = 1·375
             3 atoms oxygen. .... = 3·000
                                             ‾‾‾‾‾‾
                                             11·875

Rosin kept for some time in a state of fusion at the temperature of 276° was subjected to analysis in the same way.
I obtained 4·049 cubic inches carbonic acid = 0·518 gr. carb.
Water, 0·2 gr. .... = 0·022 gr. hyd.
                                             ‾‾‾‾‾‾
                                             0·540
Loss .... = 0·460

This loss I ascribe to oxygen.
These quantities converted into volumes become :

                    Carbon. 4   cubic inches.
                    Hydrogen. 1
                    Oxygen 1½

This is equivalent to

4 atoms carbon. .. = 3·000 or 8 atoms carbon. .. = 6·00
1 atom hydrogen.. = 0·125   2 atoms hydrogen . = 0·25
3 atoms oxygen .. = 3·000   6 atoms oxygen... = 6·00
                    ‾‾‾‾‾‾   ‾‾‾‾‾‾
                    6·125   12·25

So that by the heat two atoms of carbon and nine atoms of hydrogen are removed, while the atoms of oxygen are doubled. From this we see that a considerable proportion of oil must have been driven off by the heat.

VIII. *Morphia.*

I find the easiest method of obtaining morphia in a state of purity is the following: Into a strong infusion of opium pour caustic ammonia. Separate the brownish-white precipitate by the filter. Evaporate the infusion to about one-sixth of its volume, and mix the concentrated liquid with more ammonia. A new deposit of impure morphia is obtained. Let the whole of this deposit be collected on the filter, and washed with cold water. When well drained, pour a little alcohol on it, and let the alcoholic liquid pass through the filter. It will carry off a good deal of the colouring matter, and very little of the morphia. Dissolve the impure morphia thus obtained in acetic acid, and mix the solution, which has a very deep-brown colour, with a sufficient quantity of ivory black. This mixture is to be frequently agitated for 24 hours, and then thrown upon the filter. The liquid passes through quite colourless. If ammonia be now dropped into it, pure morphia falls in the state of a white powder. If we dissolve this precipitate in alcohol, and evaporate that liquid slowly, we obtain the morphia in pretty regular crystals. It is perfectly white, has a pearly lustre, is destitute of smell, but has an intensely bitter taste, and the shape of the crystals in all my trials was a four-sided rectangular prism.

When one grain of pure morphia is passed slowly through red-hot peroxide of copper, it is converted entirely into carbonic acid and water. The water obtained in four successive experiments was always 0·5 gr. The carbonic acid gas amounted to 3·58 cubic inches, supposing the barometer to stand at 30 inches, and the thermometer at 60°.

Now 0·5 gr. water contains. .... 0·0555 gr. hydrogen
3·58 cubic inches of car. acid 0·4528 gr. carbon
_____
Total .... = 0·5083

There is wanting 0·4917 gr. to make up the original weight of the morphia. This deficiency must be owing to the morphia containing a quantity of oxygen equal to it in weight.

It follows, from the preceding data, that the constituents of morphia are as follows :

Hydrogen. ., ... 0·0555
Carbon. .... 0·4528
Oxygen. .... 0·4917
_____
1·0000

Now when these weights are changed into volumes, they amount very nearly to

18 volumes hydrogen,
24 volumes carbon,
10 volumes oxygen.

This is equivalent to

18 atoms hydrogen.....	=	2·25	5·59
24 atoms carbon.	=	18·00	44·72
20 atoms oxygen.	=	20·00	49·69
		40·25	100·00

So that, if the preceding analysis be correct, the weight of an integrant particle of morphia is 40·25.

The reader will easily perceive that we might consider this substance as a compound of only half the preceding number of atoms, or of

9 atoms hydrogen	=	1·125
12 atoms carbon	=	9·000
10 atoms oxygen.................	=	10·000
		20·125

On that supposition the equivalent number for it would only be 20·125. Perhaps this last estimate may be the most correct; but the analyses of the salts of morphia, published by Robiquet and by Pelletier and Caventou, give an equivalent number for morphia not far short of 40. This is my reason for considering it as a compound of 62 atoms rather than of 31 ; either of which is equally indicated by the analysis.

IX. *New Projection of the Sphere.* By Capt. J. Vetch, R. E.

In this projection the globe is supposed to be inscribed in a cylinder, the axes of the globe and cylinder being at right angles to each other, and their surfaces, therefore, coinciding at a meridian. The eye is supposed to remain at rest in the centre of the globe, and each point in the earth's surface is transferred to that of the cylinder by a right line passing from the earth's centre through that point. The cylinder being then unravelled, a view of the earth is obtained on a plane surface. A sketch of the earth's surface upon this projection has been published by the author, accompanied by a short account of its principles.

X. *Excrement of the Chamæleonis Vulgaris.* By Dr. Prout.

A small portion of the excrements of this animal was given me for examination by Dr. Leach. It consisted partly of a fine powder, of a bright lemon-yellow colour, and partly of lumps composed of the same powder loosely agglutinated. On examination, it was found to be chiefly composed of the lithate or urate of ammonia, and a little colouring matter. Hence, as indeed I had expected, its composition was precisely the same as that of the urinary excrement of the boa constrictor and lizard tribe, as previously ascertained by Dr. J. Davy, and myself. The food of this animal is said to consist of the lumbricus terrestris, and the larvæ of the tenebria molitor.

Article XI.

Magnetical and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·27″ North. Longitude West in time 1′ 20·93″.

Magnetical Observations, 1820. — Variation West.

.Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
April 1	8ʰ 35′	24° 29′ 59″		1ʰ 15′	24° 38′ 49″		—ʰ —′	—° —′ —″	
2	8 40	24 30 28		1 35	24 38 12		6 30	24 32 52	
3	8 35	24 28 33		1 05	24 42 23		— —	— — —	
4	8 40	24 32 24		1 20	24 39 41		6 30	24 28 12	
5	8 35	24 31 28		1 15	24 41 20		6 25	24 24 27	
6	8 45	24 32 00		1 25	24 41 27		6 30	24 26 32	
7	8 35	24 30 06		1 25	24 40 41		6 25	24 33 42	
8	8 40	24 35 13		1 15	24 41 29		— —	— — —	
9	8 40	24 32 00		1 30	24 38 45		6 30	24 32 38	
10	8 40	24 30 44		1 20	24 38 11		6 25	24 33 27	
11	8 05	24 29 45		— —	— — —		— —	— — —	
12	8 45	24 29 44		1 15	24 40 09		6 30	24 32 18	
13	8 35	24 31 21		1 20	24 41 04		6 35	24 32 47	
14	8 40	24 29 30		1 25	24 42 33		— —	— — —	
15	8 35	24 29 23		1 25	24 41 44		6 45	24 33 17	
16	8 35	24 30 56		1 35	24 40 03		6 05	24 34 45	
17	8 35	24 28 04		1 35	24 40 55		6 55	24 33 02	
18	8 35	24 31 10		— —	— — —		— —	— — —	
19	8 35	24 29 05		1 25	24 42 38		6 50	24 32 52	
20	8 35	24 33 36		1 20	24 42 54		— —	— — —	
21	8 45	24 29 55		1 20	24 40 36		6 50	24 32 58	
22	8 35	24 30 33		1 20	24 41 · 36		6 45	24 33 57	
23	8 45	24 30 26		1 30	24 40 28		6 50	24 33 04	
24	8 40	24 30 56		1 25	24 39 00		— —	— — —	
25	8 35	24 30 58		1 25	24 39 03		6 55	24 33 40	
26	8 40	24 29 12		1 25	24 40 00		6 25	24 31 59	
27	8 35	24 30 28		1 25	24 42 22		— —	— — —	
28	8 40	24 32 33		1 20	24 37 44		6 50	24 32 32	
29	8 35	24 29 05		1 20	24 37 38		6 55	24 33 05	
30	8 35	24 29 32		1 50	24 42 04		6 55	24 29 10	
Mean for the Month.	8 37	24 30 38		1 24	24 40 29		6 37	24 31 58	

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
April		Inches.				Feet.		
	Morn....	29·554	43°	81°	WSW		Foggy	38½
1	Noon....	29·488	55	55	W by S		Fine	55⅔
	Even	—	—	—	—		—	44½
	Morn,...	29·609	49	72	W by S		Cloudy	
2	Noon....	29·605	60	63	WNW		Cloudy	60½
	Even	29·623	55	65	W		Fine	49½
	Morn....	29·729	53	49	NE		Cloudy	
3	Noon....	29·729	59	60	Var.		Cloudy	59½
	Even	—	—	—	—		—	39
	Morn....	29·493	46	60	SE by E		Fine	
4	Noon....	29·444	59	41	SSE		Very fine	60⅔
	Even	29·400	53	47	E by S		Fine	43½
	Morn....	29·344	51	49	SE		Very fine	
5	Noon....	29·303	62	43	SSW		Cloudy	64½
	Even	29·188	58	48	SW by S		Very fine	44
	Morn....	28·936	48	87	SW		Rain	
6	Noon....	28·910	50	63	W by S		Showery	51½
	Even	28·910	43	70	WSW		Showery	31
	Morn....	28·980	47	63	W		Fine	
7	Noon....	28·984	45	54	W by S		Sn. show.	47½
	Even	29·000	41	68	SW		Showery	35
	Morn....	28·893	41	66	SSW		Fine	
8	Noon....	28·805	49	60	S		Cloudy	50⅓
	Even	28·757	—	77	E		Rain	41
	Morn....	28·788	41	87	NW		Rain	
9	Noon ...	28·880	48	63	WNW		Cloudy	49½
	Even	28·954	44	66	W		Showery	33
	Morn....	29·013	40	79	SSW		Cloudy	
10	Noon....	28·940	45	80	SSE		Rain	47
	Even	28·878	46	87	SSW		Rain	45
	Morn....	28·990	47	85	SSW		Cloudy	
11	Noon....	—	—	—	—		—	56
	Even	—	—	—	—		—	44½
	Morn...	29·272	49	82	SW		Cloudy	
12	Noon....	29·323	55	64	Var.		Fine	56
	Even	29·363	52	72	SSE		Showery	44½
	Morn....	29·404	47	83	NE		Fog	
13	Noon....	29·400	53	71	Var.		Rain	53
	Even	29·344	49	80	NE		Rain	45
	Morn....	29·133	46	90	NE by N		Rain	
14	Noon....	29·127	51	75	NNW		Showery	52
	Even	29·146	—	83	NW		Showery	37½
	Morn....	29·309	45	61	NW		Clear	
15	Noon....	29·335	50	59	Var.		Showery	54
	Even	29·395	48	54	NW		Fine	38
	Morn....	29·583	46	58	SW		Fine	
16	Noon....	29·609	58	56	WNW		Fine	58⅔
	Even ...	29·635	55	60	NW		Fine	46¾
	Morn....	29·764	56	58	W		Very fine	
17	Noon....	29·762	63	55	NNE		Very fine	65½
	Even	29·743	58	61	NNE		Very fine	49⅔
	Morn....	29·712	55	64	ENE		Very fine	
18	Noon....	—	—	—	—		Very fine	65
	Even	—	—	—	—		Very fine	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
April		Inches.				Feet.		
19	Morn....	29·705	55°	58°	NE		Very fine	48¼
	Noon....	29·674	64	54	Var.		Fine	67½
	Even	29·633	62	58	WNW		Fine	46
20	Morn....	29·673	54	56	NW		Very fine	
	Noon....	29·713	61	54	NW		Cloudy	61½
	Even....	—	—	—	—		—	45
21	Morn....	29·820	53	54	NNE		Clear	
	Noon....	29·797	61	50	Var.		Very fine	63½
	Even....	29·795	57	55	E by S		Very fine	46
22	Morn....	29·892	53	56	E		Clear	
	Noon....	29·903	62	53	E		Clear	63
	Even....	29·892	56	56	E by N		Clear	40
23	Morn....	30·013	52	58	ENE		Clear	
	Noon....	30·019	62	50	ENE		Clear	63
	Even ...	30·039	55	57	NE by E		Clear	40
24	Morn....	30·053	47	64	NNE		Very fine	
	Noon....	30·053	62	52	NE		Very fine	63
	Even....	—	—	—	—		—	38
25	Morn....	29·958	48	64	NE		Fine	
	Noon....	29·900	59	53	NE		Clear	60
	Even....	29·849	53	57	NE		Clear	40
26	Morn....	29·489	51	57	WSW		Very fine	
	Noon....	29·338	60	52	NW by W		Cloudy	62
	Even....	29·254	49	58	NW		Rain	39
27	Morn....	29·235	40	81	N by E		Rain	
	Noon....	29·321	41	65	NE		Showery	43
	Even....	—	—	—	—		—	36
28	Morn....	29·528	42	57	N by W		Cloudy	
	Noon....	29·546	51	52	Var.		Very fine	53
	Even	29·547	47	54	SW		Very fine	36½
29	Morn....	29·615	46	57	SW by W		Very fine	
	Noon....	29·622	55	50	SSW		Fine	57½
	Even	29·624	50	53	W by S		Cloudy	42½
30	Morn....	29·696	48	70	NW		Showery	
	Noon....	29·739	56	53	NNW		Fine	58
	Even	29·780	52	53	N		Fine	

Rain, by the pluviameter, between noon the 1st of April, and noon the 1st of May, 1·505 inch. The quantity that fell on the roof of my observatory, during the same period, 1·495 inch. Evaporation, between noon the 1st of April, and noon the 1st of May, 3·75 inches.

ARTICLE XII.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
4th Mo.								
April 1	S W	30·10	30·04	62	45	—		87
2	N W	30·23	30·10	66	49	—		74
3	N	30·23	30·12	64	36	—		79
4	S E	30·12	29·88	65	31	—		73
5	Var.	29·88	29·41	72	45	57	21	68
6	S W	29·44	29·36	56	32	—	11	89
7	S W	29·57	29·41	52	31	—		75
8	S W	29·41	29·27	54	44	—	—	76
9	N W	29·46	29·27	52	28	—	10	86
10	S E	29·44	29·37	52	46	—	14	88
11	S W	29·78	29·44	59	45	45	17	89
12	S E	29·91	29·78	59	44	—	—	81
13	N E	29·88	29·68	53	45	—	32	93
14	N E	29·84	29·71	55	39	—	25	96
15	N W	30·12	29·84	59	30	20		83
16	S W	30·27	30·12	63	43	—		73
17	N W	30·22	30·18	68	39	—		82
18	S E	30·20	30·14	69	37	30		83
19	N W	30·18	30·14	72	44	—		80
20	N W	30·31	30·18	65	35	—		72
21	N W	30·39	30·31	68	32	—		74
22	E	30·51	30·39	65	34	47		72
23	E	30·53	30·51	65	35	—		69
24	N E	30·51	30·47	66	36	—		71
25	N E	30·47	30·03	63	29	50		72
26	W	30·03	29·77	69	40	—	28	68
27	N E	30·07	29·92	46	36	—		74
28	N	30·15	30·07	52	34	—		67
29	S W	30·21	30·15	60	37	—		68
30	N W	30·36	30·21	63	28	46	—	75
		30·53	29·27	72	28	2·95	1·58	96—67

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month.—1. Cloudy: windy. 2. Cloudy. 3. Calm: close: overcast. 4. *Cirrus: Cirrocumulus:* clear. 5. *Cirrus: Cirrocumulus.* 6. Cloudy: showers. 7. Hoar-frost: some gentle showers during the day: a few flakes of snow, p. m. 8. Hoar-frost: cloudy: showers. 9. Showery: fine. 10. Windy morning: heavy squalls, with showers, most of the day: some thunder clouds, and a rainbow, p. m. 11. Showery. 12. Overcast. 13, 14. Rainy. 15. Fine: *Cirrus: Cirrocumulus.* 16—24. Fine, with *Cirrus* at intervals. 25, 26. Cloudy. 27. Some gentle rain this morning: the wind strong and cold from N E. The swallows made their appearance about five this morning in great numbers. 28, 29. Fine. 30. A gentle shower about nine, a. m.

RESULTS.

Winds: N, 2; NE, 5; NW, 8; W, 1; SW, 7; SE, 4; E, 2; Var. 1.

Barometer: Mean height

        For the month. .... 29·984 inches.
        For the lunar period, ending the 6th. .... 29·974
        For 13 days, ending the 10th (moon south) .... 29·790
        For 14 days, ending the 24th (moon north) .... 30·127

Thermometer: Mean height

        For the month.................................. 49·385°
        For the lunar period, ending the 6th. .... 48·258
        For 30 days, the sun in Aries. . ... 48·883

Hygrometer: Mean for the month .... 77·9
Evaporation.. 2·95 inch.
Rain .... 1·58

Laboratory, Stratford, Fifth Month, 22, 1820.   L. HOWARD.

⁎⁎⁎ A letter received from a friend in *Philadelphia,* says, under date *Fourth Month,* 3d, " After some days of fine spring weather, we yesterday had a snow storm of 10 hours continuance, which covered the ground about five inches deep ; but the weather is again mild, and the snow has nearly disappeared."

The reader is desired to compare with this account the changes from warmth to cold experienced *with us* in the early part of this month ; and, in particular, the depression of the temperature by night (or minimum) between the 2d and 3d ; and that by day, between the 5th and 6th of the month, which was continued through several days following.

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END OF VOL. XV.